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EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY

S. W. FERNBERGER, UNIV. OF PENNSYLVANIA (*J. of Exper. Psychol.*)
W. S. HUNTER, CLARK UNIVERSITY (*Index*)
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The Quest for Constants: HARVEY CARR, 514.

The Biosocial Nature of the Personality Trait: P. E. VERNON, 533.

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THE PSYCHOLOGICAL REVIEW

THE UNITS OF EXPERIENCE—MEANING OR GESTALT[1]

BY W. B. PILLSBURY

University of Michigan

Psychology is perhaps not the only science in which there is at present disagreement as to just what its subject matter is. On the face of it that a science should not know what it is dealing with seems somewhat absurd. Look a little deeper and the absurdity partly disappears, for the differences are not so much about what shall be treated as they are about what we shall call the material we are to discuss. My purpose is not to consider the relative merits of all of the suggestions that are current as to how best to formulate the subject matter of psychology. I shall limit myself to a discussion of the comparative merits of two suggestions as to what we immediately perceive or think. The problem verges on philosophy since it asks how we know and how we think of things.

Psychology is in a position peculiar among the sciences in that the material with which it deals can be regarded either as what it is in itself or what it represents or means. When you see this desk before you, we know from physiology that you have certain impressions upon your retina. We can determine fairly definitely what they are. The striking second fact is that you are yourself not at all concerned with those images. The desk you see is here many feet in front of you while the image is in your eye. Your experience is of the desk as here, not of the something in the eye. We know also that it is very much distorted in the images of the retina, while

[1] A lecture on The Henry Russell Foundation, University of Michigan.

you see a perfect desk here in front. We could make an indefinitely long list of differences between the image and the interpretation that you spontaneously and automatically give it. You do not know anything of the image as we know you must have it. You are also not at all aware of the processes, if any there are, that you make use of to correct and interpret the image. You merely see the desk at once. You have no knowledge of the complicated process which may be assumed to intervene between the image and the accepted interpretation we call the object.

It would be too long a story to trace all of the steps that have been necessary before we came to the formulation of this problem. At first it was assumed that the objects as seen were really counterparts of the sensations and brain processes. Then it was thought that what were called sensations, the immediate results of sensory stimulations, were received, retained, and put together to make the object as it was seen. That when thus constructed the appreciation of the object or the object as appreciated was merely a sum of these discrete sensations. Gradually there developed an appreciation that such an interpretation did not do justice to the facts.

Brentano was among the first to point out that the sensory processes as presented and the thing that we intended were quite distinct and in many cases very dissimilar. He distinguished then between sensation or idea that might be regarded as the structure immediately in mind and the thing outside that was really referred to or meant. You mean the desk as a solid piece of furniture. You have on your retina and presumably in your consciousness in the primitive sense of the term merely an elongated mass of brownish sensations, double in part, single in part. Brentano insisted that it was characteristic of the mental as opposed to the physical that it should always have these two aspects. We sometimes call them the structure, that is just the mass of stimuli, and the thing which is meant. The latter is the meaning of the structure. It is as if one were using signal flags. The flags are nothing but arrangements of cloths of different shapes and colors. The message they may give that a ship is lost has a

very pronounced effect emotionally and otherwise. The structure is the flag, the message is the meaning that they impart. In mental life we neglect the structures and are aware only of the meaning. In crises the same may hold of the signals. We are not aware of the flags. We appreciate only the tragedy they report.

It is this double aspect of mental processes that marks them as peculiar. On the one hand they are just themselves, on the other they are what they refer to or mean. A difficulty with all interpretation is that we are aware only of the meaning and of the final interpretation. We know of the existences of the images or structures only from theoretical considerations and from the fact that in illusions the structural phases break through the meaning to distort them. On the whole we accept the meaning as real. If one is to identify any mental process with the external object it must be the meaning. Our thought is always of the object as in space outside and of memories as of the real events in the past. We are not aware that we have images on the retinal surface and then project them outwards. We do not even know that we have a retina until we study physiology. We are certainly not aware of any process of projection. Similarly when thinking of last year's commencement, we do not have memory images and then date them back to last June. We merely picture the scene as of last June.

Since the original discovery of Brentano that we must distinguish between the mental content and the fact of representation, many attempts have been made to explain how the two processes are related. Some even raise the question whether there are two processes. One of the most used and most contested points of attack was suggested by an Austrian philosopher, Ehrenfels, in 1890. He amplified a suggestion of the Vienna physicist Mach that in addition to the merely sensory components of any sensation or other experience there was also a unitary factor in the total that seemed to be immediately experienced. It remained if the sensory components were varied in any way and might on occasion itself vary without any change in the sensory parts that were pres-

ent. Thus a melody will be recognized if transposed to another key. In that case the notes will be altogether different, but the effect will be approximately the same. On the other hand a square standing on one corner gives an entirely different effect from the same square standing on its base. Ehrenfels asserted that this something different was a real factor to be regarded as different from and either added to or suggested by the elements. He called it the Form or Gestalt. He insisted that the real essence of any perception was to be found in the Gestalt. He asserted that it could be regarded as the really important phase of the percept and went farther to advocate the view that all truly characteristic phases or processes of mind were just these Gestalten or forms.

That it is a contribution to the problem of meaning raised by Brentano lies in the fact that the Gestalt for Ehrenfels was the true essence of each percept or of each memory or thought. The figure of the square is one meaning of the group of four lines just as the image represented or recalled by the word constitutes a part of the meaning of the word. It is what they mean for us over and above the bare four lines themselves. Some twenty years after Ehrenfels wrote, the modern Gestalt School discovered his article and took over his name and many of the points of his doctrine. They have changed some features of his theory, but have maintained its essentials. In our discussion we may use the term Gestalt to apply to the doctrine of the group as a whole. The later men modified Ehrenfels not merely to make the Gestalt the meaning of the figure, but to assert that the Gestalt was all that there was in the process. It was a combination of both sensation and its reference. In general they tend to go farther than Ehrenfels and assert that there was at any time nothing but the Gestalt. The sensory elements had no real existence, or could be neglected. The desk as experienced was all there was. The stimulus might possibly be the occasion for the origin of the form, but it had no existence except as it constituted part of that form. They sometimes deny that a form has any parts; so the sensation would cease to be part of the explanation.

It is interesting to note that Köhler in Chapter III of

'Gestalt Psychology' calls the view of the Introspectionist the 'meaning' theory. He has obvious reference to Titchener's denial of existence to meaning and his classification of the meaning phase of experience as the 'stimulus error.' Historically it would be more correct to assert that starting with Brentano's distinction between the immediate content and its outward reference, Titchener denies absolutely the meaning or reference aspects, while Köhler just as definitely denies the structural aspect. It seems to the writer that meaning and Gestalt are identical. They both intend to indicate the immediate experience. Brentano saw meaning in reference to content, Köhler is not willing to admit the reference or that any distinction is possible.

For the Gestalt school experience is just a series of these units. They may have their occasion in the world outside and then we call them perceptions; they may originate spontaneously within us and then they are known as memories or processes of reasoning. We may show how Gestalt applies to the explanation of perception, for perception is regarded as the simplest of the mental processes. Other schools have asserted that perception is a result of a construction. Some stimulus is said to affect the sense organ, this stimulus arouses memories that combine with them and these all combine to give the idea we have. While these stimuli, called elements, and the final interpretation or meaning are assumed to be present, it is not claimed that the various stages in the operation are directly observed. The man who perceives knows only the final result. We are led to believe that the construction precedes only from experiment and from analogy. The Gestalt theorist denies the construction and assumes that all there is to the perception is the final product.

If the process of perception is merely arousing a Gestalt that appears at once when the stimulus acts, obviously the next question is what is a Gestalt and the second is why does one Gestalt appear at one time, another at another. Exactly what a Gestalt is has never been clearly stated. One could define it as the essence of a figure that determines exactly what the figure is. Ehrenfels suggested that it was

the determinant of the circle as ascribed to the lines, or the meaning that four dots assumed when they were approximately at the corners of a square. The Gestalt was for Ehrenfels dependent for its existence and appearance upon the presence of the dots or the lines. Köhler has asserted since, and is followed in this by most of the modern members of his school, that Gestalt is independent of the lines or other parts that compose it. It certainly determines the whole and possibly the parts themselves, the parts do not determine it.

If we accept this position, the Gestalt is something that is not definable in terms of its parts or of the conditions under which it arises. It is a thing apart, immediately known and not to be further analyzed or described. One more negative description is that the Gestalt does not develop from, nor is it modified by, experience. The evidence for this last point may be considered in detail later. Suffice it to say now that a part of a figure is not more easily recognized as part of a figure because it has been many times appreciated alone. Slightly less negative is the consideration that a Gestalt is not limited to any sense. The figure of the visual percept gave the original name, but, as was said, a melody is also a Gestalt, as the awareness of movement in vision or on the skin or of a sound is a Gestalt. Wherever there is pattern or organization it is present and is the determiner of the organization. Also a Gestalt is not restricted to presentations of sense. We recall Gestalten. In recall they are not merely repetitions of the old and familiar, but are also new organizations of familiar material. The ideas used in or developed through thoughts, too, are called Gestalten. In fact the popular words idea or notion in their broadest sense become alternative designations for the Gestalt.

If these be the negative and general characterizations of the Gestalt, it is obviously highly desirable that we give some positive characteristics or designations. The very generality makes this difficult. They seem to be immediate units of experience. If this be true, it should be possible to enumerate them if they are not capable of description. Occasional illustrations have been given by the upholders of the theory.

Thus a circle is said to be a Gestalt, so also is a square and a triangle. Gestalten may be divided into good or perfect Gestalten and poor or imperfect Gestalten. Experiment indicates which are better and which are worse. Thus if figures be shown with a brief exposure or with an illumination so faint as to make the figures uncertain, it is noticed that there is a tendency for certain figures to be seen when they are only suggested and for others to be transformed toward these figures. If an ellipse is shown, it is likely to be seen as a circle. By all tests the circle is the best and most persistent Gestalt, next in order come the hexagon, the square and the equilateral triangle.

If these figures are shown with breaks in the lines, the lines are filled in to perfect figures, if the exposure be brief or the intensity of the light be dim. When the gaps become too large, the parts of the figures break up to form figures of their own. Each of these tends to approach some more perfect Gestalt or whole. A Gestalt is said to tend to complete itself. Where choice is offered, a good Gestalt will dominate a poor one. The measure of good is the degree of domination. After all, however, no one has made more than the most modest beginnings of an enumeration of the Gestalten. These have value only as illustrations. Nothing has been attempted either in giving the natural history or development of a Gestalt. In fact, as was said, it is denied that a Gestalt has any history. At least experience is said not to alter the Gestalt in any way.

The evidence for this was supplied by Gottschaldt. Gottschaldt showed series of simple figures a very large number of times. After this the simple figure would be included as a concealed part in an elaborate one and this would also be shown a number of times and the observer was asked to describe the whole. If the second figure was shown with no special request to seek out the small figure that had been seen before, but merely with the request to describe it, the observers did not note the presence of the small figure in more than one and two per cent of the cases. When they were asked to look for the repeatedly seen element in the larger

whole, the percentage of times in which it was noted rose, but only to nineteen per cent. It was found also that the number of times the smaller figure was shown did not have a direct relation to the likelihood with which it would be seen. The number of successes when the part figure had had ninety-two previous exposures was just as large as when the single figure was exposed 520 or 540 times.

It is questionable whether Gottschaldt's results prove more than that when a figure is observed with the intention of seeing it in one way, that seeing does not predispose the individual to see it when one is looking in another way or with another intention. Gottschaldt's complicated figures were so very complex that hundreds of different simple outlines could be seen in them. The figures that were obvious were so complete in themselves and the small forms so divided between separate parts of the larger that the smaller are not recognized in them. In short, the experiments do not show that the Gestalt is developed through experience. It proves merely that seeing a figure alone does not make it more easy to see when it is a part of an organized larger whole, especially when it is not a separate or readily separable part of that whole. Also the experiments show that repeated repetitions do not increase the readiness of recognition after a minimum capacity for recognition has been reached.

This theory of the Gestalt, then, insists that our experience is nothing more than a series of these units which parade through us without our knowing from whence they come or whither they go. They are asserted to be sharply broken off from one another, each closed within itself. The psychologist is not permitted to analyze them, or to study their origins. They come and we accept them. It makes no difference whether we are watching events in the world outside, recalling events from the past, or reasoning about events that are to come, we are dealing always with these units. We have no expectation of making a complete list of them, there is no value in grouping them into classes, or in applying any of the methods of the natural sciences to them. They are because they are. They are to be accepted just because they present themselves and without asking their warrant.

Some members of the school have asserted that they are principles of organization with the implication that they have elements which are analyzed. Some have even asserted that they are the results of wider processes of organization of the medium or whole in which they are found, but this seems to be a schismatic doctrine and is not accepted by the better recognized leaders. The emphasis upon organization does not seem to be compatible with the opinions of the stricter sectaries, although the emphasis upon it by groups spurred on by the school has proved valuable in the explanation of subsidiary problems. The notion of organization was developed in many other schools before the Gestalt theory became prominent, and is more consistent with the fundamental doctrines of other schools.

If Gestalt is limited as an explanation from the fact that it seems merely to assert that there are units of experience rather than to say what they are or how they originate, it is desirable to consider alternative interpretations as to what the unit may be. The first possibility is an adaptation of the Brentano doctrine of meanings and of the original von Ehrenfels notion of Gestalt. At any moment of perception, we must consider the objective situation and the accepted interpretation that it arouses. The interpretation is all that the onlooker knows. It is what we accept as the real thing. The interpretation is like the squareness added to the four dots, or the melody aroused by the notes in Ehrenfels' illustration. Where several interpretations may be made, as proven by the fact that they are made, to the same objective situation on different occasions or by different individuals, we must assume and can prove that the interpretation made depends upon the experience of the individual and upon the attitude that controls him at the moment. The course of thinking is determined by similar conditions.

That the interpretation added varies with experience can be shown by the increase in accuracy of interpretation from infancy to adult life. It is proven also by the failure to interpret many new situations. Experimental evidence and observation of chance incidents prove that the interpretation

varies with the situation. In Italy, moving the hand towards the body or face means farewell, in America the same movement means come. How it is interpreted depends upon both the training of the onlooker and where he happens to be. We judge between a concave and a convex surface by the shadows, but how the shadow is interpreted depends upon which direction we assume the light to be coming from. We make the interpretation automatically without thought as to the direction from which the light is coming. The different meanings of words under different contexts are quite as striking in the more intellectual processes. *Hell* in a German sentence has a different meaning from that which it has in English. *Bat*, *bar*, *lead* and almost all simple words change their meanings almost as completely from context to context. Under the influence of the situations and the reader's attitude the interpretation comes at once and is accepted as the real event without knowledge that it involves any mental process or mechanism whatever.

If we study different cases it becomes evident that we may distinguish two different ways in which the situations are interpreted. In the first class there seems to be little that is pictorial added to the situation or stimulus. This applies to the interpretation of words. Words are merely seen or heard and the meanings arise as part of the total experience, not as anything added to the stimulus. They do depend upon the setting or context. *Hard* as applied to a man and to a mineral arouses entirely different nuances, but each meaning arises immediately and as part of the total effect. With such a noun as *bat* there may be pictures, the flying mammal in one case, the plaything of the pitcher in the other. Even these pictures need not really present themselves. The word plus the awareness that one could have the picture or some more general notion may be all that constitutes the meaning.

In perception just as little indication may be present of the processes that determine the meaning. Thus you appreciate the distance I am from you by means of double images received one from each eye, and by other similar slight circumstances in the way the eyes receive the impression, which have

been discovered by years of painstaking experimentation. None of these factors are noticed for themselves, but together they have come to mean for you that the object is at a certain distance from you. The result is a unit for experience, there is no direct awareness of the conditions of the interpretation, but we know that they must coöperate to produce the effect.

In other instances, it seems that some previously developed construction is added to the effects of the original stimulation of the senses. It may be presented quite as immediately as in the former case in which we call it meaning. The two sorts of interpretations probably shade over somewhat gradually into each other. In earlier writings I referred to this developed construction as a 'type' and argued that it was an outgrowth of earlier tests by all the senses and arose from checking memories against immediate sensations until some construction was obtained that fitted the requirements of experience and would harmonize with other types. We cannot in all instances see how each experience acts to change the type, but we can show that it varies as the individual's observations increase. I suggested under the analogy of the simpler sorts of motor learning that these types might be said to develop under the influence of trial and error. The first awareness might be a mere image and this might be modified by later images and by comparison with memories of earlier images. The first tentative interpretation would satisfy certain tests, but fail to satisfy others. With each failure, the interpretation that failed would be rejected or it would be modified and other trials made until one was found that satisfied all the conditions and gave a picture consistent with the sum total of experiences of that object or situation. This would survive and so be accepted as real.

While a theory of this type needs to be supplemented by study of each experience or form or Gestalt, if one prefers to steal a term from the school we are criticizing, it does encourage such a study. It gives some value to experience and offers the possibility of making clear the origin or explanation of experiences. It also harmonizes with the fact that one does not see how any experience is modified or what its

components are. We do not rationally patch sensation by sensation or sensation by memory until we find a satisfactory garment for them later. In fact the Gestaltist is right, we do not see sensations directly. We infer the particular moment. In the adult most of this patching must have been done long before the individual was aware of what was going on. Occasionally we can see how an interpretation changes. Usually the completed product alone is appreciated. The interpretation does harmonize with the results of Crosland's study of the change in memories as they are subject to the passage of time. In memories the inconsistent parts are dropped. What is left is supplemented with new material that harmonizes with the remnant. The accepted memory is a resultant of elimination as well as of addition and transformation. This too is accomplished without the intention or even the specific awareness of the man who develops the memory.

On this theory an object as we see it is the resultant of all of the experiences we have had of the object or situation that is concerned. In this we learn to omit all of the factors that have proven themselves not to belong. We do not see the blood vessels which are in the interior coats of the retina and which we must look through to see any object. We learned long ago when we tried to touch these shadows of blood vessels that they were not real. Soon we disregarded them and now ignore them so completely that only special experimental conditions enable us to see them. We ignore differences in color due to the part of the retina upon which the light falls. We ignore after-images except under special conditions. We do not notice complementary colors and shadows for we know the object is not fundamentally affected by them. On the positive side we correct the acute and obtuse angles of man-made objects, because we know that the carpenter is addicted to right angles and we are safe in assuming that any article of furniture will be rectangular. The corrections have been made through a long series of experiences and are the outcome of many tests in which only the satisfactory survive. When we have built up a satisfactory standard object in each class we see that standard in place of the immediate image

that is given in sensation. We have the two alternatives; the elementary experience may be a Gestalt or it may be a type.

If we must choose between assuming that the elements of experience are immediately given, independent of experience, inexplicable in every way, and assuming that they are the products of living, of testing and using the organs upon objects of all sorts, the first question that arises is what is the basis of choice. We admit that evidence must be indirect. No one has seen a type develop and probably no experiments can be devised that would enable us to start with the undeveloped sense impressions and see them grow by separate trials into the finished percept that we call the object. On the other hand the researches of Gottschaldt that were devised to prove that experience had no part in developing an appreciation of form are hopelessly ill suited to the purpose. They prove the dominating influence of purpose and setting in perception, that one cannot see a figure in one connection when everything in the figure and in the observer compels him to look at it in another way, but nothing more. They show that one observation under suitable attitude is more important than hundreds when the attitude is not adapted to seeing one particular figure or part of a figure. They do not prove that experience has not an important influence in building up the form that is seen in each of the particular situations.

If we are to take a more general basis of decision, it is helpful to ask which would be the more valuable as an explanation if it happened to be true. Judged on this criterion, the Gestalt explanation is not especially successful. It is merely an assertion that mental life is made up of a succession of units of experiences that came we do not know from where and concerning which we do not know why or how they return. We are not permitted to break them up into anything more fundamental. No description is given of them, no enumeration of the different classes into which they may fall. One may even object that some of the attributes ascribed to them are contradictory. Thus they are said not to be composed of elements, but again are said to be thoroughly organized.

How there can be an organization with nothing to organize is a question. Sometimes this is varied to assert that the whole controls and determines the parts rather than the parts the whole. This makes organization possible, but still leaves the figure somewhat mixed. The whole would be a self-dependent entity and an overlord of the parts. It is not enlightening as to the nature of experience and gives no basis for prophecy. If the negative assertions were capable of proof the demonstration might outweigh practical considerations. Lacking complete demonstration a demand for a pragmatic warrant should have a hearing.

Again the postulates of the Gestaltists preclude any explanation of how or why mental events succeed one another. The old explanation of association so long prevalent they abandoned because it seemed to involve elements to connect and they banned elements. They have put nothing in its place. In consequence Gestalten just happen along. In thinking it is said that a problem is solved when an unsuitable Gestalt is resolved into a suitable one. Again there is no indication of how or why resolution takes place. It just occurs. The theory says nothing of how we know a good solution when it appears. They speak in animal experiments of insight. An ape has an insight that makes it possible for him to put together two bits of bamboo to reach a banana which is too far away to be reached by either alone. Nothing more is said of insight than that it is a new Gestalt which chances to appear.

If one may summarize, while the Gestalt theory recognizes a problem, and points out some imperfections in the older views, it has done relatively little to solve them. The holders of the Gestalt theory deserve credit for much painstaking experiment. Its defects grow from its one-sided extension of its complete criticism.

It seems possible to combine some of its best features with a modified formulation of the doctrines it neglects and obtain a much more useful and complete view. We may assume with the Gestalt doctrine that the object meant is what is known immediately. We can regard this unit of meaning or

concept as a product of experience, not as derived from nowhere. The experiences again are not elementary sensations, so far as the individual knows them. On the contrary they are first mere vague awarenesses which become clear and ordered through use and by trial. These separate elements, call them 'types' or Gestalten, are not given in isolation, but are part of a larger unit or organization. As far as they arise without external stimulation they have been developed in a larger pattern as Peterson first, and then Shepard, showed that rats developed the learning of a maze. The immediately preceding figure in the pattern is not only effective in determining the succession in the pattern, but the succession is a result of much wider organizations.

The rats as they run a maze gradually build up this pattern probably without awareness of special turns. They omit false openings and, no matter how often they may have gone the longer way, gradually take the shorter. The construction of the pattern is not by mere connection of next to next, but by the becoming clear of the path as a whole. It is not merely a motor process; for when gates prevent taking a short cut, the rat by attempting to take the forbidden shorter way may show his awareness of its existence. These patterns are developed in the process of living. They are organizations in which not merely the contiguous elements determine the course, but all of the experiences near and remote in their proper proportion. We could speak of trial and error as in the formation of the concept, provided only we understand that the trials are not noticed as separate elements, but are gradual developments under the influence of the repeated trials. It is an organization of the vague and diffuse into the definite and specific.

We can assume also that something occurs in the organization of the series of events that are revived in memory. These may be regarded as patterns of life history. They repeat themselves with variations in larger and smaller patterns. Bits of separate larger patterns are selected for combination in the revivals and woven into new patterns under the influence of the special circumstances. This assumption saves the

essential fact in the older association explanation, that experience in some way determines the order of return of events and the solution of new problems. At the same time it eliminates the objectionable feature, the assumption that elements were determining and that connections were between isolated elements. In the solution of new problems at least and in lesser degree in the reconstruction of old experiences, there is again much of the tentative. Just as the rat organizes a pattern out of many successes and failures in the discovery of his path, so the thinker has an inkling here and a suggestion there of the way he is to solve his problem. Finally these various attempts find themselves organized into the true solution.

These patterns themselves are to be regarded as outgrowths of experience. They are essential to ordered reliving and new construction of the mental life. These new constructions and ordered reliving of events are not to be regarded as a fortuitous succession of Gestalten, but they are expressions of the results of earlier succession and of the wider organization of previous groups in the series. We are not ready to trace all of the factors. Mere succession of discrete elements is not adequate to explain the course, but we cannot leave them unexplained and with no hope of explanation.

As opposed to the Gestalt notion, too, we must see that the separate temporally succeeding units are not absolutely distinct. Each dissolves into the next and the separateness that we ascribe to them is more a matter of later reference than of actual distinctness. We need points of reference in speaking of them and in recalling them. They are not discrete as experienced. It is the reverse of the moving picture film. There a series of separate frames blends into the continuous succession of events. In our living there is a continuous succession of events as they are experienced, but we divide them into successive units in later recall for convenience of report.

The Gestaltist asserts that one should accept as unitary the immediate whole as experienced and should not look behind it. We may accept his statement on this point. If we

are to go far we must insist, however, that there is some way of tracing the connections between these units to determine why they appear as they do. When we trace them we see evidence that earlier experiences do control the reappearance, and that they are to be understood as part of a wider organization. This may at present be indefinite, for not all of the laws have been traced. It is much better to be indefinite and leave a hope of discovering a real explanation than to be content with the statement that the mental units merely succeed one another without any possibility of discovering an order in their succession or saying why one comes at one time and another at another. If one must choose as our unit of experience between a Gestalt that cannot be further described or defined and a meaning which is dependent upon previous experience and organization, the meaning or type is obviously to be preferred.

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THE GHOSTLY TRADITION AND THE DESCRIPTIVE CATEGORIES OF PSYCHOLOGY[1]

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Beside a Paleolithic camp fire lies a sadly wounded hunter. One can see him, touch him. Yet something momentous has happened to him, for he can no longer move. Something clearly has departed from him. Belief in the soul is thus born when man dies.

All this is a commonplace of our intellectual history. Not so commonplace is the realization that the soul of man thus early discovered has had a long and intricate evolution. A brief résumé of that evolution will serve to orient us to certain considerations to be later advanced. In early thought as seen in the Homeric epics and in early Christian apocalyptics, the soul after death was merely a slightly attenuated and bloodless copy of the living man. It hunted, ate and slept, loved and hated, fought and suffered just as the living man did. The acts of the living and the acts of the soul were distinct occurrences but without necessary distinction in kind. Nor were the activities or functions of *living* man as yet divided into two kinds, the bodily and the mental or spiritual. That discovery came later.

About 500 years before the Christian era, however, meddlesome thinkers began to ask what the soul was for, and what it was like. Because the corpse left behind at death is obviously of the earth earthy, we find these philosophers presently contrasting mind or soul with body or matter as two distinct *kinds* of reality. The naïve dualism of *existences* became a dualism of *quality*.

It took philosophy nearly 1500 years to develop and clarify what it meant by this dualism. As is well known,

[1] A revision of the Presidential Address at the Eighth Annual Meeting of the Midwestern Psychological Association.

Descartes achieved a final and conclusive statement with his celebrated definitions of matter as that which occupies space; of mind as that which is conscious. The two modes of reality were at last conceived as immeasurably and utterly distinct. Nor does there seem much hope of escape from some such a chasm once one starts down the dualistic road. With good reason therefore is Descartes taken as a turning point in the history of philosophy.

There is another excellent reason why we may halt our survey with Descartes' achievement. No philosophy much under 400 years of age is of practical importance, since it affects people's thinking only when they are overtly philosophizing. But *old* philosophies do matter. They get embodied in the vocabulary, if not of the man in the street, at least of all cultured persons.[2] Presently they emerge as 'commonsense' and hence not to be challenged by sensible persons. Yet challenge them we must. The effect of the ghostly tradition upon psychology is such that we must suspect nearly every obvious way of speaking about our phenomena.

It would, for example, conduce enormously to clearness if we could leave out of our technical thinking all such terms as soul or mind or consciousness and most or all the terms which derive from these dualistic concepts. We might then return to the naturalism, implicit in Aristotle, which inquires how living creatures, human or otherwise, differ from non-living things and from one another.

Two closely related difficulties, however, stand in the road of this delightful simplification. The first is the fact just alluded to, that these terms have got themselves very nicely embedded in our vocabulary. Waving a magician's wand and pronouncing them metaphysical does not dispossess them from this strongly entrenched position. In short, plain people and even psychologists, indeed even behaviorists, *will* use them.

[2] Thus all the languages of the West express the same mind-body dualism, but there are interesting differences which strikingly reveal the influence of the vocabulary. An unbiased examination of Indian and Chinese originals should be suggestive here.

The second difficulty is this: that the terms 'bodily' and 'mental,' though primarily used to express a metaphysical distinction between two kinds of reality or being, are also used to express a difference between two sorts of facts. When I speak of 'mental processes' one thinks of such activities as remembering and forgetting, perceiving and comparing, imagining, striving, feeling emotion, while the words 'bodily processes' suggest such functions as digesting, breathing, glandular secreting, muscular contracting.

It is not my contention that these groups of processes are altogether properly contrasted. They have been chiseled out of the crude matrix of experience with the aid of terms primarily designed, not for the impartial observation of facts but for the expression of a fine-spun metaphysics. When we label an act as bodily or mental we tend, because of the ghostly tradition, to pass from a simple division of two kinds of acts over into the philosophical assumption that the human being has a dual nature. That is, we are always tempted to assume that the set of activities called 'bodily' are to be ascribed to a thing or material entity called a 'body' whereas 'mental' activities are similarly to be ascribed to an entity called mind. We are tempted and we nearly always fall.

When in the course of time the entity 'mind' is discredited, psychology is left with no criterion for the identification of the activities which it studies. The result is a sad confusion in the descriptive categories with which it works. This confusion, together with certain others of related origin, is a chief factor in retarding and sterilizing research in psychology. The situation resembles that in alchemy in the seventeenth and eighteenth centuries. Concepts like phlogiston and attraction were not wholly wrong but the science leapt forward when they were replaced. A similar revolution in theoretical physics is perhaps the outstanding intellectual landmark of our day. Psychology is unprogressive today less for lack of a great man as suggested by Boring than for lack of adequate conceptual tools.

In search for these descriptive categories, therefore, we shall set forth a certain postulate as to the nature of the

so-called mental processes which attempts to avoid the metaphysics of the ghostly tradition, shall consider one or two critical implications thereof, and finally show that the acceptance of this view points the way towards a more vigorous and pragmatic description of the facts of psychology.

We may start by noticing that both the processes traditionally called mental and those—equally traditionally—called bodily take place within or as functions of a visible living organism. The psychological or psychic, not less than the physiological, is an activity of anatomical structures. (This last somewhat clumsy expression, anatomical structures, is preferred to the term organ or tissue to avoid the implication that the structures are gathered in one place.) Now the activities of these structures have been described as consisting in motions—motions in glands, nerves or muscles. It is the uncritical acceptance of this view that activity and motion are identical which constitutes the *credo* of behaviorism.

It is probably true, as this doctrine implies, that a complete analysis of the activity of an organism may be made in terms just of the transportation of masses through space, but such analysis fails to bring to light, precisely because it is analysis, the integrational characteristics of the activity in question, the characteristics belonging to the activity as a whole. It is these characteristics of an integrated activity which belong to psychology.[3]

[3] Prof. Niels Bohr, the distinguished physicist, has recently taken a parallel position with regard to the relation of the inorganic to the living. We shall never discover, he believes, a sort of life energy over and above the energy of physical science. Yet we can also never measure life in terms of physical processes without some remainder. For measurement implies instruments external to the thing measured—that is, instruments which in measuring do not affect that object. Obviously the measurement of physical processes in the living organism, carried beyond a certain point, kills the organism. A similar difficulty in subatomic physics, however, led to Heisenberg's principle of indeterminacy, and this proved to be no mere assertion of an impossibility but the essential postulate of a new and exceedingly fruitful mode of analysis. Professor Bohr predicts the same opening of new vistas for biology when life itself is accepted as a sort of factor of indeterminacy. (See N. Bohr, 'Licht und Leben,' *Naturwissenschaften*, 1933, 21, 245–250.)

It will be recalled, moreover, that the new quantum physics by no means abandons all of the classic physics, though it rejects its essential postulates. The parallel with our own proposals is very close.

That an activity as a whole may have qualities which are not disclosed by its analysis into motion is not a new doctrine derived from *Gestalt*, though the form in which it is here stated has been influenced by that school. At any rate, I do not understand Gestalters to employ their favorite concept of wholeness as criterion of the psychological. Quite on the contrary, both Köhler and Wheeler delight in finding Gestalten 'here and there and everywhere.' I am, on the other hand, postulating that those events which we have traditionally but somewhat erratically classified as mental or psychic or psychologic are precisely the activities viewed as integrated wholes of living anatomical structures.

Neither is this the position taken by J. B. Watson *et al.* that psychology studies the activities of the integrated whole organism while physiology studies the functions of parts of the whole. We must distinguish between an activity considered as a whole and an activity regarded as a function of a whole organism. Of course, there is a sense in which the organism is an integrated whole, and everything done by the organism, whether called physiological or psychological, is therefore a function of that unified whole. But except in some such vague and harmless sense, relatively few psychological activities represent a unified or integrated whole organism. To pass by automatisms and split-personalities *et hoc genus omne*, I fail to see that an uncontrolled burst of anger which brings to ruin a long-prepared plan of action for some passionately desired end is any more the expression of the integrated organism than is the orderly oxygen interchange of the blood. Surely, no matter what our criterion of integration, it is less so.

Nor have I ever encountered a physiologist willing to have the psychologist take over all activities of the integrated organism. I for my part am unwilling to surrender wholly to physiology certain activities which seem undeniably functions of restricted portions of the organism—visual perception, for example. Only by introducing a teleological *motif*—clearly a strange proceeding for a behaviorist *de sang pur*—can this be regarded as an activity of the whole organism.

Despite purely verbal similarity, therefore, our position is almost the converse of the behaviorists at this point. For we hold that the psychological no less than the physiological is an activity not of the undivided whole organism but of specific, though not necessarily compactly localized, structures or parts within the organism. The difference between the two kinds of processes lies in the fact that the psychological is the activity taken as a unified whole.

This position is, of course, not axiomatic. It is, rather, frankly a postulate. The validity of a postulate must be gauged by its scientific fruitfulness. This really means in the backward state of the science that men will choose their postulates very much as they choose their neckties—not wholly without reference to the sequels or implications but never quite wholly because these are critical or determinative. Less cynically, perhaps, we might say that scientific fruitfulness is to be judged in two ways: by the intellectual clarity the postulate affords, and by the amount of research it seems to stimulate. At bottom these are doubtless much the same thing but at any given stage of progress they may generally be distinguished. Let us, therefore, first consider in the light of our postulate that *pons asinorum* of any systematic psychology the so-called mind-body relationship, the ancient origin of which we have just traced. The banishment of the mind-body problem from psychology has been often proclaimed but never achieved. It belongs truly enough to philosophy but the form of its expulsion from psychology profoundly influences (when it does not distort) what remains in that science.

A survey of the wrecks attending modern efforts to solve this problem reveals clearly that fatality attends any attempted solution which errs in any one of three ways. First, it must not violently deny, or seem to deny, the unique character of the phenomena which have traditionally been called conscious or mental; secondly, it must not run violently counter to the current natural science world-picture; and, finally, it must not leave 'mental phenomena' outside the stream of other natural events, uninfluenced by and without influence upon them. The first is obviously violated by be-

haviorism, and despite the enormous influence which it has exercised upon the thinking of us all, I believe that that doctrine is now receding rapidly as a distinct point of view. Interactionism violates the second, parallelism and most forms of double-aspectism the third criterion.

The postulate here advocated enables us to meet all three criteria. Its very basis, of course, is the acceptance of the first by positing a class of phenomena roughly corresponding with the traditional mental processes—though the right is reserved to redefine and reclassify. The second and third criteria present us with the problem of effectively relating these mental events to physiological processes without upsetting current natural science.

In our unguarded moments all of us are dualists—the ghostly tradition again—and at least one-way interactionists. Mental events depend, we imply or say, on the physiological. This is a careless and confusing statement. There are two kinds at least of dependence.

That which is signified by the words 'comparing this red and that green' is, I take it, a mental event. It depends on, in the sense of being constituted by or made up of, a variety of motions in sense organs, glands, nerves, muscles. Without these it would not occur. Certainly that is dependence, but it is not causal dependence. It is something so different that the concept of cause-and-effect simply does not apply here at all. It is here that we part company with traditional interactionism. In considering the relation of a mental event to those particular physiological processes which constitute it, causality is an irrelevance.

This does not mean that between physiological and psychological no causal relations obtain. It will help us when we consider in what way physiological and psychological processes *are* causally related if we recall that both kinds of process are activities of anatomical structures. Now what can causally influence the activity of an anatomical structure? Obviously, either some extra-organic, external, energy-source or the activity of some other anatomical structure. Thus comparing two colors is directly influenced by the external

energies of the light sources. But if one has been taking santonin or mescal, the comparison will also be effectively influenced by the activity of certain other anatomical structures—the digestive organs, the blood, and I know not what besides. This is a familiar instance of a physiological process affecting a mental event. No one has ever effectually denied this kind of influence since the time at least of La Mettrie or really disbelieved in it. Our postulate makes room for it.

No less does it take care of the reverse effect, the influence of a mental event on physiological process. This is the real crux. Here is where one risks the charge of mysticism. Well, let one of us be driving very fast when he suddenly, very suddenly, notices that the traffic light is not green but red. This is a case of simple awareness, a mental event if that term can have any distinctive meaning. Suppose now that our friend, W. B. Cannon, were to test our splanchnic blood. He would find, would he not, clear evidence of physiologic change —a change really consequent upon the awareness of color change. Do we not then have here a mental-physiological causal sequence or, to put the matter bluntly, a mental event causally influencing a physiological process?

To say this would not be to lay one open to the charge of mysticism if it were not for the ghostly tradition which leads us all, spiritualists and materialists alike, to divorce mental events from their context as activities of visible anatomical structures. For all that we have asserted is that the activity of one structure or group of structures—the activity of comparing colors—has led to the activity of other structures in the viscera. As a matter of fact, not only is this conceivable; it is inconceivable that the functioning of one organic structure should fail to influence other and subsequent functions of the organism.

And there you are. Like a competent magician I have pulled out of my hat the complete interaction I put in with my postulate. I claim nothing more. That is, I claim merely that the postulate makes intelligible a world in which mental processes may affect the physiological and vice versa. By restoring mental events to their natural context as organic

functions, we make intelligible the fact that such processes or events play their part and are played upon by the world of physical events. Yet nowhere in this interaction have we cut across the closed circle of energy interchanges postulated by current physical science. Nowhere have we implied the intrusion of non-physical energies or the intervention of ghostly powers. I believe this point of view as effectually banishes ghosts from psychology as any physical monism or so-called materialism.

The practical value as well as a limitation of this position become apparent if we proceed with our analysis a little further. If we are correct in identifying the mental event with the activity taken as an integrated whole, then it is clear that this mental event is likely in most cases to be the most distinctive and important part of the causal chain. Thus to hold to our illustration, while mescal intoxication may be considered a physiological process and a cause of changes in one's color perception, it is at least equally legitimate and useful to consider the total act of eating mescal as the cause of the changes. This total act as such we hold to be psychological in character.

In saying this we are of course implying that the scientific investigator of organic activity, like any other investigator, possesses a certain freedom of choice—nay rather, is compelled to exercise a certain degree of choice—in seeking the causal antecedents of a given event. This choice is not arbitrary but determined by the ends the investigator is pursuing. If the distinctive thing about a complex network of cause-and-effect relationships is a component part of the antecedent organic activity, then we call that cause physiological. If, on the contrary, the distinctive thing is the activity considered as an integrated whole, if the quality or characteristic of the event as a whole is critical, then we call the cause psychological. And similarly for the effects. Otherwise stated, if it gives us greater control over the course of events to manipulate and alter primarily a certain component part-activity (though obviously this involves alteration in the total complex) the cause is to be regarded as physiological. If it gives us greater

control over the course of events to alter or eliminate the integrated activity as such, then the cause is to be regarded as psychological.

It thus becomes apparent that the distinction we are making between the mental and the physiological is something of an abstraction. The real unit of living is fundamentally and always a psycho-physiological act. But to brand a distinction as an abstraction is not to say that it is either unreal or useless. The point of view, the concepts, the techniques and methodologies of the two sciences of physiology and psychology have been developed in response to the need for specialized study of these two aspects—even though they are only abstractly separable—of the unitary activities of the living creatures.

Abstractions, however, may have an historical validity without being currently very useful. I have just shown that if the abstract distinction be drawn as I have drawn it, a reasonable theory of mind-body relation is possible. That is, no doubt, an interesting *tour de force* but if there are no further consequences, a rather sterile one. I have wearied you thus far in the belief that the adoption of this postulate will also and in consequence facilitate a fresh attack upon the question of the concepts in which we shall describe and classify our facts. But even if the proposed postulate be not accepted, this question of our descriptive categories must be seen as a critical one. W. S. Hunter's assertion, in his presidential address before the American Psychological Association at Toronto, that probably all these categories are already known is surely but the counsel of despair.[4] How many of us are satisfied with the classification of the Index or the Abstracts? Is any one of us satisfied even approximately with the descriptive terms he perforce uses in the field of his own special interest? Does attention, for example, describe a group of phenomena having a certain internal consistency? Does emotion? Learning? Thought? Inhibition? Will? In-

[4] More precisely, Hunter alleges that all the forms of behavior are known but this clearly implies, for one who understands the relation between observation and description, that we have a full armamentarium of descriptive concepts.

stinct? Action? Judgment? Conation? Intelligence? Or almost any of the current terms taken at random?

The late A. P. Weiss has emphasized more sharply perhaps than any of his predecessors that the terms utilized in psychology are for the most part descriptive of facts which have unity only for the external observer, not for the reacting organism. The supposed instinct of self-preservation is a flagrant example of such a category. The observer may logically—and perhaps even usefully—classify acts as tending to preserve the life of the organism, but there is ample evidence that there is no process or unitary set of processes of any sort within the organism which corresponds to such classification. Our science is full of such categories.

We may follow Dr. Weiss in envisaging the major task of psychology as the translation of such psycho-social categories into those which are based squarely on the activities of the human or other animal organism. But we need not, and I think must not, follow him in finding such activities only in 'muscle twitches' or 'electron-proton aggregations.' Weiss and the behaviorists see no escape from this because they are resolutely determined to rid themselves of the ghostly incubus. In this effort, however, they have themselves adopted a metaphysics which, in its very opposition to dualism, is none-the-less framed in terms of an unreal distinction of mind and body.[5] Our postulate seems to enable us to steer between both Scylla and Charybdis. It provides us with categories, or at least makes a place for categories, which are neither merely external and social, nor yet merely physiological.

Thus the discovery that what we ordinarily call thinking has unity only for the external observer, that is, that there really is no single process properly called just thinking, does not force us back upon an analysis of thinking into laryngeal movements and their like; nor is such analysis, though perfectly proper, particularly illuminating. To say that thinking is performed by means mainly of subvocal move-

[5] It is said of a distinguished contemporary that all his psychology is influenced by the fact that he has a soul to save. Many of his most critical opponents might do well to remember that a psychology is also influenced if one has a soul one is determined to lose.

ments gives us almost no help in training people to think clearly. I am bound to say that the Titchnerian analysis of thinking into sensory images seems to me equally sterile, and perhaps equally correct. Neither gives us much of a toe-hold for effective control, neither lays the basis, as a sound psychology will someday do, for a practical logic.[6] Spearman's view that judgment consists mainly in the eduction of relations and correlates, on the other hand, represents an effort at a genuinely psychological approach which, if it is confirmed and refined—as I think it needs to be—by further research will help us to control the processes of judgment in ourselves and others.

It will be noted that we do not deny the validity of the subvocal analysis of thinking, nor even its qualified usefulness for the job of the psychologist. I can, for example, quite conceive that many of the aphasias may be understood in terms of localized defects in the speech apparatus. But I am quite certain that a more subtle *psychological* analysis is needed for most of them.

I have deliberately chosen this example of the aphasias because by common consent, nay by definition, they are due in every case to brain lesions. They are organic disorders. Yet the classification of aphasias in terms of localization of lesion has got nowhere. Re-education and treatment, as may be seen from Franz's studies and Head's monumental work, compel us to a psychological grouping. The aphasic reaction is to be described and classified in terms of the whole act of the person, not in terms of its constituent part-processes. The case is thus illuminating, linking what is so clearly and indisputably a matter of organic, anatomic structures with a description of their activities in psychological terms—exactly what our postulate would suggest as most likely to prove useful.

Learning is, of course, another of these rather external concepts. It is becoming increasingly apparent that there *is* no one learning process, no learning curve, no forgetting curve.

[6] Please note that I am not advocating basing the content of psychology on logic; the converse is the case. Logic is a sort of art or technology wherein the chief ingredient is psychology. The same, I may say, holds for ethics.

These are useful ways of describing an event as it appears to the outside observer; they do not describe the activities of the learner as such.

Recognition of this fact—and no student of learning today, I venture to believe, would question it—does not automatically insure description in proper terms. Only through careful experimentation and statistical treatment can we discover the functional unities of animal behavior. Such experimentation is surely not wanting; reformulations of the problem of learning are quite the order of the day. The function of the postulate here considered is to indicate the area within which the search for proper descriptions is most likely to be successful; not in the common-sense units of everyday life nor yet in the constituent physiological processes but in a region which is, in some degree, between the two.

The same case can be made more concrete perhaps in the field of emotion. The traditional categories of emotion are also either devices for classifying activities which have a common social value or they are the product of the ghostly tradition—generally both. We need not conclude, as many have done, that there are no distinct emotions or that there is no psychological problem of the emotions at all. This is to take certain experimental results far too seriously and to fly in the face of much other evidence.

Let us take Cannon's famous studies of adrenal activity uncritically, that is to say at about the level of undergraduate understanding. Certain types of emotion are in part constituted by such secretion. After the emotion itself as such subsides, the animal may remain somewhat hyperadrenalized; in terms of our postulate, you see, the integration which *is* the emotion has been lost without the disappearance of one of the constituent parts. Now it is extremely probable that such hyperadrenalism lowers the threshold for emotive stimuli. Should this turn out to be true, the analysis of emotion into its constituents would be a valuable clue in the control of emotion.

Not so valuable, however, as an understanding of what happens, for example, to the emotional relations of father and

son as a result of repeated punishments or other forms of parental domination. Here the categories of physiological analysis, however complete and genuinely relevant, must forever remain inadequate. We need genuinely integrational, psychological categories. If we are to understand, still more if we are to direct, control, and modify such relations we must find the *unified acts of the person which are as wholes capable of training or modification.*

For the discovery of these categories, we need clinical, experimental, and statistical study. But such study if improperly oriented leads often to negative or misleading results. Our postulate suggests the wisdom of the approach from above rather than the approach from below, of beginning with the confused but relatively complete categories of psychological tradition rather than with the necessarily always incomplete categories of physiology.

There is no reference here to physiological research oriented towards the solution of its own peculiar problems. I can think, for example, of few events which would be more exciting to psychologists than a convincing demonstration of the nature of the neural impulse. But physiological research and still more physiological speculation with reference to problems of behavior has been too often an exceedingly futile affair—neurological tautology, as Adolf Meyer once characterized it. Think of the literally thousands of pages devoted to the attempt to lay a neural basis for the law of frequency—an attempt foredoomed to sterility if, as now seems likely to an increasing number of investigators, there be no law of frequency. To come up under a problem by boring from below only to discover that there is no problem there is discouraging business.

Physiology has made most progress in the study of intrinsic behavior, that is, in the study of those forms of behavior whose functional unity has, in the main, been tied up with some rather definitely localized organ or organs. Here the rough-and-ready psychology of everyday observation has provided the physiologist with sufficiently well defined problems to permit a beginning. Yet even here, a more

refined psychological approach is ultimately necessary. Surely vision is a fairly unified group of functions with reasonably well-defined organs. Color vision seems even more narrowly defined; nor do I deny the validity of the attempts to discover the physiological basis thereof. But the rediscovery by Gestalt—with original experimental demonstrations—that color vision depends upon something more than the intrinsic photo-chemo-electric processes of the retina indicates the need for definition of the problem in terms of the integrated activities which we are here claiming as the field for psychology.

And even when the problem is properly set—by psychology—for a physiological analysis of behavior, the results we believe are necessarily incomplete, an abstraction made from reality which misses a significant aspect of the whole. In addition to setting certain problems for physiology, psychology must formulate and solve the problems intrinsically its own.

I believe then in beginning quite shamelessly with *practical* problems—in the case of learning, *e.g.*, with a study of how children learn in school; though for that matter, also, of how white rats act when they are matriculated in our institutions of higher learning. Fully recognizing, however, the overlapping of processes involved in such practical problems, I propose that we proceed through a series of approximations and corrections, seeking categories of description having a functional unity which will stand up under statistical and experimental examination. Such functional units we believe will lie intermediate, for the most part, between the external psycho-social categories of everyday life and the categories of physiological interest. Psychology has always in fact largely occupied itself in this intermediate region. But it has done so by aid of vestigial categories which are infected with a vicious dualism. To rid ourselves of the ghostly tradition without contrarily swinging to the opposite extreme clears the way for a straightforward search for the descriptive categories so sadly needed. We shall thus be headed for real units of behavior which will enable us accurately and completely to describe behavior and thus eventually to control it.

SUMMARY

A point of view which may be called physical mentalism is based on the postulate that the events studied by psychology take place in and are functions of anatomical structures of living organisms. They are distinguished from physiological processes (which may have the same loci) by being these functions considered as integrated wholes. (The position that *activities* may be studied as a whole is to be distinguished from the merely verbally similar view that psychology studies the activities of the whole *organism;* in behaviorism these activities of the whole organism are supposed to be analyzed, ultimately into electrons and protons, not taken as wholes.) Psychology and physiology study two abstractly distinct aspects of organic process.

This postulate provides a distinct class of 'mental phenomena' and renders intelligible their interaction with other natural phenomena without running contrary to current natural science hypotheses. The implications of the postulate in conceiving the interaction of mental processes with the physiological are considered and illustrated. Adoption of this point of view enables us to avoid the pitfalls of a metaphysical dualism which has for so long confused the terminology of psychology.

A second source of confusion in our descriptive categories is that they denote activities which have unity only for the observer, not for the reacting organism. The postulated criterion of the psychological turns us toward the descriptive units appropriate to the problems of psychology. The analytical approach—as in the reduction of thinking to laryngeal movements, etc.—is not denied value but is subordinated to the study of the dynamic relations between activities taken as wholes. The statistical, clinical, and experimental isolation of such functional units is the major problem of present-day psychology. It is held that such units are more likely to be found by beginning with the larger complexes of everyday life and simplifying these than by attempting the synthesis from below.

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THE QUEST FOR CONSTANTS[1]

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Under this enigmatic title, I propose to discuss two opposed attitudes towards experimental problems. I shall first illustrate them in terms of one of the old problems of psychology—the span of apprehension—and then discuss them in relation to some of the problems of learning.

How many spatially discrete objects can one visually apprehend at once? The question implies that there is a numerical constant involved which it is our duty to discover. Titchener (5) and Pillsbury (4) report this number to be 4 or 5. Of course these exact numbers are rarely obtained in our experiments, but the constant is there but hidden from view because of the influence of variable distractive conditions which it is impossible to eliminate or control. Our experimental procedures are thus designed to eliminate these factors in so far as it is possible, and then to uncover this hidden constant by statistical means.

The statements of Titchener and Pillsbury have been cited merely for illustrative purposes, for the attitude involved—which we shall term the *constancy* attitude—in varying degrees of strength seems to have been the orthodox one toward the majority of our experimental problems.

What shall we do with these numbers when we get them? Is their attainment the goal of our science? Will they give us scientific elation and peace of mind? Will our students be thrilled by such knowledge? Do we obtain these numerical facts in order to explain them? If the number apprehended is a constant, must not that fact be contingent upon some fixed characteristic of the optical apparatus? What are these limiting characteristics?

[1] Opening address before the Midwestern Psychological Association at Ames, Iowa, May 18, 1933.

The opposing attitude is that of *relativity*. It assumes that the number apprehended is a function of the particular conditions employed, and that the obtained value will vary with these conditions. The experimental procedure is as follows. The value for an arbitrarily selected set of conditions is first obtained. These conditions are then systematically varied in the attempt to discover which are effective and which are non-effective. The attempt is made—not to hold the number constant—but to control it as much as possible—to vary it within its maximal and minimal limits. The interest of the experimenter is not in the numbers, but in the list of effective factors and their kind and degree of influence.

Is the number of objects apprehended a constant or a highly variable quantity? To my mind the facts at hand indicate a high degree of relativity. If the objects are clear-cut, distinct, homogeneous, and arranged in a square, 36 may be readily apprehended. Perhaps this value may be still further increased by a judicious choice of conditions. The number will vary with the size and spatial separation of the objects. If we use black squares upon a white background, and gradually increase their size and spatial separation, finally only one will be visible. The number apprehended is highly relative to the conditions of the experiment.

What shall we do with this list of efficacious and non-efficacious conditions when we get them? Can we classify them in any significant way? Can we discover any unifying and general principle running through them, any simple and systematic way of conceiving of the phenomenon we are studying? Is there any guarantee of order in the midst of this variety, and what shall we do if we find a list of unrelated conditions? If they are unrelated, have we not merely succeeded in complicating that which seems to be relatively simple? Shall we simplify by seeking for constants in the midst of variety, shall we seek for unifying principles to introduce order into diversity, or shall we be content with complication?

Perhaps it is *relations* that are constant. Let us grant that the magnitude of the phenomenon we are studying—which we

may denote by X—is contingent upon a number of variable conditions, A, B, C, and D. I vary condition A quantitatively, and measure the concomitant effects upon the magnitude of X, and express this relation in mathematical terms—provided it is possible to do so—and of course a clever statistician always finds it to be possible. Perhaps it is these relations that are constant. Have we a right to assume it? Is it not equally possible that the value of the relation XA is dependent upon the other variables B, C, and D? Are the relations of X to each of the determining conditions independent of each other, or are they contingently related to each other? Are there any constants in the realm of mind, or are all things relative to each other?

This raises the question as to the truth of these two concepts in respect to any particular feature of mind. Is the value of this particular object of study independent of or relative to other variables, or is it independent to some extent and dependent in part? We do not know in advance. We may have our biasses and strong convictions on the subject, and we may assert them with confidence and a considerable degree of positiveness, but such statements do not constitute science. The constancy or relativity of a given phenomenon can not be determined on *a priori* grounds. The nature of the phenomenon must be discovered empirically.

We can, however, legitimately compare the two attitudes from the standpoint of methodological approach. Which is the better attitude to adopt in investigating the nature of a phenomenon?

If the phenomenon in question does vary with conditions, the relativity attitude will speedily uncover that fact, but the constancy attitude will not. If the phenomenon is a constant, the relativity attitude will also conclusively demonstrate that fact, while the constancy attitude will merely assume it to be true. The truth of the constancy concept can not be tested directly; it can be tested only indirectly—by proving or disproving the relativity hypothesis. The relativity attitude can thus be justified on methodological grounds.

Presumably our empirical approach to any problem should

be dominated by the relativity attitude, and yet this has rarely been the case. The general history of the experimental attack on many, but not all problems, runs somewhat as follows: The existence of constants and universals is assumed. Several experiments are performed in which the conditions differ in several supposedly unimportant respects; for how many cases can one cite in which an experiment has been repeated with an exact duplication of conditions? Naturally different numerical results are obtained. Directly contradictory results are sometimes secured. Which of these results shall we accept as the true one? We are inclined to pick and choose. The reliability of one experiment is questioned. The selected conditions in another are not to our liking. We may strike some sort of an average of those remaining, and take that for our truth, and naturally writers will differ in their choice. There is also that peculiar type of individual who dislikes to accept the work of others, and hence repeats the experiment—with some change of conditions—and takes his own results for the truth and gospel.

At this stage of development, we may find some person forced to envisage the problem somewhat timidly and hesitantly from the relativistic attitude. He assumes that the results of all the experiments are true and reliable, and hence that the differences are due to the diverse conditions. He then attempts a comparative study to isolate the determining factors. Such comparisons are usually futile, for any pair of experiments usually differ in four or five respects. Such comparisons may yield considerable in the way of theory and hypothesis, but little in the way of fact.

At this stage we may encounter some brave soul who definitely postulates relativity, and who actually performs an experiment to test the influence of some variable factor, and this example sometimes stimulates others to do likewise.

This enforced shift of experimental attitude involves a lot of waste motion. Would it not have been much more expeditious to have adopted the relativity attitude at the start? But the adoption of this attitude is usually not a whole hearted affair. There is considerable difference between *casually* in-

vestigating a few variables that happen to catch our attention, and intentionally and systematically varying all possible conditions, for we usually discover only that for which we systematically and diligently hunt.

Let me illustrate this developmental sequence of attitudes in respect to a few problems of learning. One of the early problems was that of the relation of speed of learning to rate of forgetting. But it was soon noted that speed of learning may be a function either of the materials or of the learner, and the problem came to be stated as the relation between individual differences in speed of learning and rate of forgetting.

Some measured individual differences in speed of learning in terms of the varying *amounts* of material memorized in some arbitrary unit of time. This method introduced the variables of amount of material and its degree of integration which may influence the amount retained and thus complicate the results. In other words the *numerical value* obtained may be a function of the method of measuring individual differences in learning.

The usual method measured individual differences in terms of varying amounts of *time* required to attain a specified degree of mastery of a given amount of material. A number of excellent studies of this sort have been made.

In beginning teaching most of us have to rely in part upon text book formulations until we can look up the source materials. I was nonplussed by the fact that different authors made different statements as to the nature of this relation. Some asserted a positive relation, some a negative relation, while some talked learnedly and said nothing. I was especially nonplussed by this fact, for had I not been taught that experiment solves all questions of fact in a decisive and unambiguous fashion. In looking up this literature, I found a number of studies—all of which seemed to be deserving of respect and credence. Some found a positive relation, and others a negative relation. If the distribution had been 7 to 1, the problem would have been simple, but when the distribution is approximately equal, what is a poor instructor to do? This fact led to the suspicion that the difference of results was due to a difference of conditions, and upon further inspection

I found that the positive relation was usually obtained by those who used the method of recall, while the negative relation was found by those employing the method of relearning.

The riddle was then solved with a little analysis. In the relearning method, we have but the two scores which we may term *L* and *R*. Individual differences are measured by *1/L*, and the percentage of retention by *1* minus *R/L*. The *L* scores are thus represented in the two series of values to be correlated, and they are so represented as to give a negative relation. The nature of this relation is thus not a constant, for it varies with the method of measuring retention. The various experiments also indicate that the character of the relation is contingent upon other variables as well. If the relation were systematically investigated from the relativity standpoint, I suspect that the kind and degree of the relation could be made to vary within wide limits.

What is the curve of forgetting? The question again suggests a constant. Ebbinghaus's (**2**) results were viewed with some suspicion because of the uniqueness of the results, the fact that but one subject was employed, and because of defects of procedure. The experiment was repeated several times but always with some change of conditions. The rapid initial rate of forgetting was confirmed, and this feature of the curve is quite generally accepted as a universal, though there are some facts at present that suggest scepticism. Ebbinghaus's doctrine of the permanence of slight retentive traces is also generally accepted.

In respect to the height of the curve, or the amount retained for any given time interval—three days, for example—larger values were obtained. The later experimenters differed more from Ebbinghaus than they differed from each other, and the general inclination has been to accept these later results as the typical values.

There was no inclination to assert a true or constant value for a given time interval, for Ebbinghaus himself had found that the amount retained did vary to some extent with some of his conditions. There was the early suspicion that retentiveness varies with individuals, and it was soon noted that

retentiveness varied with the method of learning, and the method of measuring retention. In spite of these variables, we were inclined to think that the results were *typical* in that they indicated within certain relatively narrow limits what might be expected to hold for the diverse conditions of life. As a consequence we were inclined to teach our students that forgetting is exceedingly rapid immediately after learning, but that nature had compensated for this fact by giving us permanent though slight after-effects. In general we were lead to believe that approximately 50 per cent of all that we learn will be forgotten in the course of three days.

The assumption of a high degree of constancy was quite in harmony with the generally accepted explanation of forgetting. Acts are retained in virtue of neural after-effects, and these neural effects disintegrate or decay with the passage of time. This disintegrative process seems to be a physiological and automatic affair, and consequently there is little that we can do about it, except to take what nature gives us. The rate of forgetting is thus an inherent characteristic of neural tissue. Brains differ in their susceptibility to impressions and rate of disintegration, and this assumption takes care of the fact of individual differences in rate of learning and rate of forgetting. Forgetting was not explained exclusively in these terms, but this was regarded as the dominant factor.

Is there a typical amount forgotten for a given interval of time? Does our acquisitive development throughout life exhibit this zigzag character—a continuous process of taking two steps forward and slipping back one every third day? If this is true, how disheartening a picture of mental development. I am thoroughly convinced of its truth, only when I grade examination papers. How nice it is to be able to blame the inexorable law of neural decay, instead of placing the onus upon our students, or of becoming unduly suspicious of the efficiency of our teaching.

Are our laboratory conditions typical of everyday conditions? Can we not vary the amount recalled all the way from zero to 100 per cent by a judicious choice of conditions? In a laboratory experiment the subject is required to memorize a

dozen pairs of nouns. The subject returns to the recall test with the knowledge that he is expected to recall the absent items that he learned three days previously. The amount recalled is obviously a function of this *mental set* at the moment of recall. There is nothing new in this observation, but it needs to be repeated, for we are generally accustomed to state our results as being independent of this condition. Such a favorable condition at the time of recall does not always obtain in everyday life.

If the subject were called in the next day and presented with a list of 100 words containing the dozen stimulus words, and informed that he is to be tested for his speed and accuracy in cancelling all d's and e's, how many of the response words would be suggested? In fact how many of the stimulus items would be recognized? Can not recall be reduced to zero?

The subject is presented with a list of ten nonsense syllables, and at recall he is confronted with the problem of reviving and identifying these ten out of a large list of possibilities. Suppose I use a list of ten common metals, or ten familiar colors, and the subject needs to identify these from a very restricted list. In fact the ten memorized items may include all of the class, and if the subject recognizes this fact, may not reproduction be perfect after a considerable interval? Do not similar conditions obtain in everyday life? Are not some impressions retained perfectly for long intervals of time, while others slip from us almost immediately? We must take into consideration the relation of the items to our previous experiences and our interests and purposes at the time of learning and recall. We know much about learning, retention and recall for our usual laboratory conditions, but relatively little of these phenomena as they occur in actual life.

Which is the better—the whole or the part method of learning? Most of the early experiments seemed to be dominated by the tacit assumption that one method must be inherently and universally the superior one. There was some variation of conditions but this was not the dominant attitude. These early experiments found that the whole was the superior method in the large majority of the cases—but there were

marked exceptions. These exceptions seemed to be regarded as statistical variants of no particular significance—or perhaps they were the exceptions that proved the rule.

On the basis of these exceptions, others assumed that the merit of the two methods is relative to the experimental conditions, and proceeded to investigate such variables as the age, training, memory span, and the I.Q. of the subject, amount of material and the number of parts in which it is divided, the distribution of practice, etc. On the assumption that both methods have advantageous as well as disadvantageous features whose relative influence varies with conditions, Pechstein (3) devised several variant methods—one of which proved to be better than either the whole or part method so far as his conditions were concerned.

The results to date indicate a considerable degree of relativity. The exact degree of relativity we do not as yet know, for the influence of all possible factors has not yet been investigated. In spite of this fact, we still find experiments with arbitrarily selected conditions which are regarded as the proper ones, and the results of which are regarded as the *true* ones.

Let me finally discuss our two attitudes in respect to the curve of learning—a topic which to my mind has received more attention than its importance justifies. What is the form of the learning curve and what is its equation? The question suggests that there is *a* learning curve whose form we should discover and describe in algebraical terms.

The constancy attitude seemed to have been the dominant one in the early experimentation, but the interest in learning was so great that we were soon deluged with a multiplicity of data. I doubt if any two of these curves are approximately identical. So far as the empirical data are concerned there is no single learning curve. Multiplicity is the rule. We can group the curves into a limited number of classes. There are the *discontinuous* curves which exhibit no uniform progression of events, and there are various kinds of discontinuity. There are those that approximate *continuity*, and here we have the negatively accelerated, the posi-

tively accelerated, the S-shaped curve, and those approximating a straight line. Can all of these be regarded as chance variations from a statistical real? They seem so diverse in form that such a conception seems hardly feasible.

In spite of this diversity, there is one theory which regards all of these empirical curves as chance samples from one single real curve. As we all know, the empirical data that we secure do not cover the entire course of learning. We do not always begin at the beginning, nor do we always carry the process to the final stage of perfection. Our empirical curves are thus samples of a larger temporal whole, and presumably the stage of mastery represented by a sample will vary with the conditions of the experiment, and to this extent they may be regarded as chance samples. The true curve is supposed to be an S curve. A sample from an early stage will give a positively accelerated curve, one from the middle stage an S-shaped curve, and one from the final stage a negatively accelerated curve.

We find here a nice expression of the constancy attitude. There is a single curve of learning which is a constant and universal and which can be expressed in mathematical terms. The usual explanation of the fact is consonant with the hypothesis. Why should the process of acquisition proceed in this sequential manner? It is an *inherent* characteristic of our neural constitution and it is usually conceived of in terms of bio-chemistry.

The truth of this theory can not be settled on *a priori* grounds. Its truth must be tested empirically in terms of the relevant factual data already at hand, and if this is not sufficiently conclusive we must arrange our experimental work to this end. We must recognize the fact that the truth of the constancy hypothesis can only be established by the method of exclusion.

Are there any relevant facts at hand? We have a multitude of experiments, but they differ in so many conditions that comparisons are often futile. They furnish much in the way of theory and hypothesis but little in the way of fact. Let us survey a few of the pertinent data at hand.

The phenomena of discontinuous trends, such as the plateau phenomenon, are not consistent with the theory that all empirical curves are samples from a single S curve. Are these representative of the nature of this particular learning process, or are they chance statistical deviates from the true value? There are but three ways of deciding. The first is to repeat the experiment, but this cannot be done upon the same subject, and repetition upon another subject may not be decisive because there is always the possibility of individual differences in this respect. Second, can this inconsistent trend in the results be correlated with some particular feature of the learning situation at this stage of progress? Such correlations that are made are largely speculative, and can not be taken too seriously. The only practical criterion is the length and consistency of the inconsistent trend. The daily ups and downs of a curve are regarded as chance variations. This is an assumption, although a probable one, for there is the possibility that some of these may be real and genuine aspects of the learning. If the values deviate in the same direction for several days, the probabilities are less, and when the trend persists for several weeks, the probabilities favor a genuine deviation. Bryan and Harter's (1) plateau persisted for three weeks, and it has been usually regarded as an inherent part of this stage of learning. In later experiments similar trends of 5 to 8 days were noted. Some termed these plateaus, while others labelled them resting or breathing places, and others regarded them as chance variations of no significance.

The existence of these discontinuous trends is thus pretty much a matter of personal opinion. I can only express my opinion that there are many curves that exhibit genuine trends that are inconsistent with any mathematically ideal curve. These are not always plateaus—periods of no apparent progress. They may occur in the early or later stages of practice, and deviate from the general trend of the results in diverse ways. I may mention a curve obtained from one rat in mastering a simple alternation problem. Progress was measured in terms of the percentage of cases in which his

selection of the two paths conformed to the objective sequence. The initial value was approximately 50 per cent, and this value steadily declined for 500 trials, and then the number rapidly increased to over 90 per cent. This curve is obviously not a section of a prolonged S curve. An analysis of the data showed that during the early stages the rat was making progress in alternating his choices, but opposite to that of the objective sequence. If these results had been scored in terms of the amount of alternation, a radically different curve would have been obtained from the same data. In other words the nature of the curve is a function of the criterion of progress.

In mathematically idealizing our empirical data, what shall we do when we encounter a genuine discontinuous trend? The usual method, I suspect, is to treat these data as chance variations, but this method is unjustifiable, because it rankly distorts the facts. The second method is to ignore the data and construct the ideal curve on the basis of the remaining data as though the entire progress exhibited this general trend. This procedure may be justified if we specifically note the fact, and take it into account in our subsequent conclusions. There is some tendency, however, for these initial assumptions to gradually drop out of consideration in the process of formulating our conclusions.

The main objection to the constancy conception is the fact that we can demonstrate that the nature of the curve is actually relative to a number of variable conditions. Let us take typewriting as an example. How do we measure progress? We secure samples of typewriting behavior for various successive stages of practice, and the progressive changes in these samples constitute the fact of learning. The samples compared are behavior events.

Let me illustrate the situation by means of spatially discrete objects. I select a number of objects and arrange them in a row, and I now measure and state the changes involved as I go from object to object from left to right. I will plot these values in reference to the positions of the objects as my abscissae values. I obtain a graph for length, height, width, surface area, volume, weight, hardness, compressibility,

brightness, electrical conductivity, chemical composition, etc. How do the objects of this series differ from one another? Is there *one* progression of differences or a *multitude* of such? The progressive changes between the objects can be truly described only in terms of all of these values, but not in terms of their arithmetic sum or of their mean. Each one of these series of values is a true and accurate measure of the progressive changes of one attributive characteristic, but not of the objects as a whole. No one series of values can be selected as the *truest* and most *typical* measure of the whole. They are all equally true and real, for each measures a particular variable, while the objects are complex, and exhibit a great variety of variables. These diverse variables may be positively or negatively correlated with each other or they may exhibit no correlation. The only necessary relation obtaining between them is that of coexistence.

The same situation obtains for our behavior samples. They are complex events, and they may be compared in great variety of ways. We may measure the progression of changes in respect to speed, accuracy, grace and ease of performance, total amount of energy impinging upon the keys, oxygen intake, CO_2 output, neuromuscular pattern, etc. We must state all of these changes in order to describe what has happened to our successive samples. What has happened cannot be measured in terms of the sum of these changes, their mean, nor in terms of any one variable or selected group of variables. Each is a true measure of one aspect of what has happened—but nothing more. No one is the measure of the whole, and no one is a truer and more typical measure of the whole than is any other. These diverse features that have been measured are independent variables, and if all of these sets of values are plotted against practice radically distinct curves will be obtained. Not all of them are S-shaped.

Psychologists are not accustomed to study samples of behavior in the all around objective manner which has been suggested. We are accustomed to select and choose in accordance with social and practical values, and hence we study the above samples in respect to *speed* and *accuracy* alone.

Is there one curve or many curves of learning? I first wish, in all seriousness, to propose the question: What is learning, or more specifically, what is the process of learning to typewrite, which we wish to measure and study? Is it one thing, or many things? Definitions are arbitrary, and we can select a definition and on this basis proceed to our conclusions. But we are accustomed to use terms with rather definite implications without defining them. What is the learning process? Learning at least is a progressive change in our samples, but what change or changes? Does learning to type consist of the progressive changes in speed and accuracy, or of something more? Does it consist of a more inclusive group of these measurable features of our behavior samples, and if so what particular group? Or does it consist of *all possible* measurable characteristics? Or does the learning process consist of *something different* than any one or any group of these measurable features, some unmeasurable unknown. If so our measurable changes in speed, and accuracy, and other characteristics are merely indices or resultants of some unknown and presumably non-measurable change or changes which constitute the real learning process.

Let me illustrate the possibilities by the plateau phenomenon. During the plateau, there is no change in speed and accuracy. If learning consists of these two changes, then by definition no learning occurs during this period. The second conception assumes that learning consists of a more inclusive group of changes—speed, accuracy, and characteristics *A* and *B* for example. Speed and accuracy are not changing, but *A* and *B* are. Speed and accuracy on the one hand and *A* and *B* on the other are independent variables to some extent, and hence the one group can change while the other does not. Speed and accuracy changes are a part of the total learning process, but not all of it. Some learning thus goes on during this period, but we fail to register it, because our data are only partial and incomplete measures of it. They measure only certain features of a more inclusive complex whole. According to the third assumption, the learning process is something different from and lies back of our four

measurable changes. These changes are merely expressions, manifestations, and indices of the real process that goes on behind the surface. It is the old question of the distinction between an *object* and its *attributes*. An object has attributes, and hence what is the object as distinct from its attributes?

Let us consider these possibilities. First, the learning process is something distinct from its measurable attributive changes. The nature of this process and its quantitative relation to each of the measurable changes are unknown. But this unknown is the real learning process whose curve is S-shaped, provided we could measure it. Well, what of it? The conception may be gratifying and mentally soothing, but that is its only value. It is not a useful scientific conception, and I doubt if any one will defend it irrespective of our tacit longing for simplicities and universals and reals.

The second conception is more feasible. Learning is arbitrarily defined as a selected group of attributive changes. Learning thus consists of this group of relatively uncorrelated changes. The true measure of learning is not the sum of these values, their mean, nor is any one value truer or more typical than another. Learning is a complex affair, and there is no *single* learning curve. Learning must be represented by a group of unrelated curves, and one curve is not truer or more typical than another, nor can a single true curve be obtained by the method of averages, for what is the significance of an average of such diverse things as speed, accuracy, grace of movement, and energy output?

It makes little practical difference whether we speak of learning to type speedily as distinct from learning to type accurately, or of learning to type speedily and accurately. In one case we are speaking of two unitary but related learning processes and in the other of one process having two attributive characteristics. On either assumption all learning curves are not S-shaped, for speed and accuracy curves always differ, and in this case the differences can not be explained on the basis of samples from *different stages* of learning.

The truth is that we are always dealing with several associated but relatively independent variables in our learning

studies. We secure several curves which are not alike. Not all can be parts of the same ideal S curve. Each is a true measure of the relation of that variable to our practice standard. There are just as many curves as there are learning variables.

The number of diverse curves secured from the same behavior samples may be still further amplified. Any curve expresses a relation between two variables—improvement and practice—and often these variables can be measured in several and equally valid ways. Take the practice variable for example. Our practice periods may be equated in terms of *amount* of work performed, or in terms of *time*. If we learn, the two sets of values will necessarily differ, and we can choose either for our abscissa. If I plot the same improvement variable against each of these two abscissae values, different graphs will be obtained.

Likewise let us take *speed* as our improvement variable which we wish to plot against one of our two sets of practice values. Speed is not a simple and unitary thing. It is a relation between two things—amount and time. It can be measured in two ways—amount per unit of time, and time per unit of amount. The two symbols are A/T, and T/A respectively, and it may be noted that the two values are reciprocals of each other. Two sets of reciprocal values plotted against the same abscissae values will *necessarily* give differently shaped graphs. Let us take such a series of integer values as 1, 2, 4, 7, 11, and 16. The differences between adjacent pairs are our *increments* of progress, and these are 1, 2, 3, 4, and 5. The curve thus exhibits marked positive acceleration. In the reciprocal set of values, the *increments* of progress will be the *difference* between the above pairs divided by their *product*. The product will increase much faster than the difference, and hence the increments will decrease, rather than increase. The numerical increments of progress for the reciprocals of the above integers are approximately 1/2, 1/4, 1/9, 1/19, and 1/35. The translation of the first set of values into their reciprocals thus transforms a positively accelerated curve into a distinctly negatively accelerated curve.

In dealing with the relation of two such variables as improvement in speed to practice, we find that each variable may be legitimately measured in two different ways, and hence we can obtain four curves instead of one. Which of these four is the true curve? All four are true and equally true. Curves of learning are not only relative to the improvement variable selected, but they are also relative to the method of measuring improvement and practice. Such curves express the relation between highly specific conditions and they have no *universality* beyond those conditions.

The form of the learning curve can be shown to be relative to many other conditions. These facts prove that it is not a 100 per cent constant. They do not prove that the constancy conception is 100 per cent wrong. The curve may be a constant or universal within wide limits, but this fact can not be *assumed;* it must be proved by experimentally demonstrating that it is *independent* of certain variable conditions, and hence is *universal* for those conditions. Amount of memory material has been well studied, and it has been found that the form of the curve is independent of this variable. Kjerstad varied a considerable number of factors without materially modifying the form of the memory curve. Apparently both conceptions of the nature of the curve are true in part. Its form is a function of certain conditions and independent of others. As to the relative degree to which it is dependent upon and independent of variable conditions, we do not as yet know.

Let me now return to one of our original questions. If we adopt the relativity attitude, and search for a list of effective conditions, what shall we do when we find them? Let me illustrate in terms of the old reaction time experiment. This problem was not approached distinctly from the constancy attitude, for we started off on the assumption of individual differences, and we early found that the reaction time was highly relative to the experimental conditions. We have here a wealth of experiment. What are the effective factors? Certainly the nature and intensity of the stimulus, the sense organ affected, all the instructional conditions of the

experiment, and the corresponding organic preparation or set, the motor organ involved, the character of the movement requested, the reacting mechanism, the extraneous and intra-organic sensory environment, and perhaps the native and acquired constitution of the organism. Practically all possible marked variations in any of the conditions can be shown to be effective factors.

What shall we do with such a list? Is it just a hodge-podge of unrelated facts? Shall we have our students memorize such a list, and call this knowledge? I know of some who assert that the total outcome of all of this experimental work on reaction time is just nil. They assert that we have merely obtained a list of unrelated and isolated facts which can not be subsumed under any unifying or general principles —that they contribute nothing to our systematic conceptions of mental life.

I have been able to derive one general principle from these diverse facts. The speed of a voluntary act exhibits a high degree of relativity. Such an act is a much more complicated phenomenon than it appears to be on casual inspection. What happens in one sensori-neuro-motor segment is a function of the present state and past history of that segment, but it is also a function to some extent of the concomitant activity and past history of all other segments. The experiments prove the truth of that recent discovery that the brain acts primarily as a whole—that the brain is primarily a coordinating and integrating mechanism in which all of its present and past activities are brought into relation with each other.

The charge is often made that psychology has over-analyzed and over-simplified its data. The charge has never disturbed me, for I do not think it is true. I have never been entirely convinced that psychology has done much analyzing and simplifying at all. We were all human beings before we were psychologists, and as human beings we used our minds in dealing with the complexities of the world. In this process our experiences necessarily become differentiated in highly complex ways. As psychologists we note and describe these differentiations, and take credit for what nature has given us. As psychologists we have contributed but little to the process.

In our practical dealings with other people we are forced to adopt a simplified and analytical attitude toward their behavior in order to deal with it successfully. We deal with acts from certain particular standpoints, and we develop a technique of describing acts in terms of their significant features. We describe a given act in terms of hunger, walking home, and eating lunch. This is a very simple characterization of the significant features—from the practical point of view—of an exceedingly complicated set of events. When we turn psychologist, we are accustomed to conceive of mental phenomena in such simple terms, and our students come to us with such concepts. Our analytical units, and our terms used to describe and identify such units are mainly of popular origin. Such terms as memory, reasoning, pleasure, rage, will, abilities, perseverance, etc., were not developed by psychologists. Our descriptions of mental events are over-simplified, but psychology is not wholly responsible for the fact.

Is it not the purpose of science to describe truly and adequately? If so, we should correct these simplified conceptions of mental events and give a truer insight and appreciation of their essential complexity—to introduce complexity into that which seems to be exceedingly simple, but to describe this complexity in terms of general principles.

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THE BIOSOCIAL NATURE OF THE PERSONALITY TRAIT

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I. The Concept of Personality Traits

Personality traits bulk so large in human intercourse that the psychologist cannot ignore them. Any remarks that one individual makes about another are usually studded with descriptive epithets, for example, 'energetic, irritable, well-meaning, pessimistic,' and so on. And if the layman cannot get on without them in his dealings with people, then it is the social psychologist's duty to subject them to scientific analysis and investigation. Although extensive attempts have been made, of recent years, to measure personality traits, yet an accepted theory of their psychological nature is still lacking. In particular we need to examine the implications of the colloquial usage of these 'trait-names.' What does the layman mean when he says that so-and-so is energetic, or irritable, etc.; upon what are his judgments of irritability based; what are his grounds for assuming that the trait is a true description at all times and in all places, *i.e.* that the irritable person is consistently irritable? The object of the present inquiry is not so much to arrive at a comprehensive abstract definition of the trait, as to stress certain aspects of its concrete usage in everyday life. These aspects have received insufficient attention in most previous theories; but, until they are included, it will scarcely be possible either to develop a complete account of the trait, or to achieve satisfactory methods of measurement in the field of personality.

According to such objective evidence as has been gathered in the last ten years or so, the trait is a most unsatisfactory concept. It is equivocal in meaning, since no one can define irritability or any other trait accurately. It is not a true description, since no two people in judging a third agree pre-

cisely on the traits which they attribute to him. And its consistency or generality is very dubious, for different tests or ratings of the same trait seldom inter-correlate at all highly. An individual who is irritable according to one criterion or in one context does not necessarily show the same trait in a different context or according to some other criterion. For these reasons a large number of contemporary psychologists desire to dispense with the concept altogether, and to substitute purely behavioristic terms in their investigations of personality. By so doing, however, they tend to lose contact with the concrete phenomena of human nature, for rigorous objectivity is a blind alley in psychology. The essentials of personality disappear as soon as the psychologist tries to translate them into strictly scientific categories. Should he exclude every vestige of *interpretation* of human actions, he would be left with nothing but physical measurements of the dimensions of the body, and of the speed, extent and direction of its movements. In fact the more objective his method of approach, the less relevant are his findings to personality. To take one example: the ordinary man, conversing with a friend, can judge readily enough from gestures and facial play that his friend is annoyed at some remark that he has made; he therefore says something appropriate and judges, by the same means, that the other is mollified. But the behavioristic social psychologist subjects a number of individuals to standardized situations which have been selected so as to arouse 'emotion'; he photographs the consequent muscular contractions in the faces of these individuals, and arrives at the purely negative conclusion that no particular conformation of the facial muscles is consistently associated with any particular stimulus situation.[1] In other words, the meaningful expression of emotions, upon which we continually rely in everyday life, vanishes when it is resolved into objective description and measurement.

All investigations of personality are confronted with this dilemma. For instance, in Thomas' observational studies of

[1] Cf. C. Landis, Studies of emotional reactions: II. General behavior and facial expression, *J. Comp. Psychol.*, 1924, 4, 447–509.

children, the elementary units of behavior such as positions and movements were recorded with very high accuracy. Unfortunately such records tell us scarcely anything of significance about the children's personalities. When, on the other hand, these movements were classified as 'social contacts' or other *meaningful* kinds of activity, the accuracy of observation was greatly diminished.[2] Even those who most staunchly assert that personality must be defined and studied in terms of habits of behavior inevitably lapse into the use of interpretative trait-names as soon as they deal with real human individuals.[3]

It seems then that the trait cannot be disowned; it is an ubiquitous component of the concrete phenomena with which the social and the applied psychologists have to deal. Let us face the facts and do the best we can to salvage it for use in a systematic psychology of personality.

II. The Biosocial View of Traits

Though objectivity, apparently, is unable to tell us anything about an individual's personality, the disadvantage of interpretation is that it cannot tell us about less than two persons, since the personality of the one who makes the interpretation is invariably bound up in his account of the other. Let us consider any two individuals, *A* and *B*, who are mutually acquainted. In the course of their contacts with one another, each builds up what may be called a *schema* of the other's personality. *B*'s schema of *A* is based on everything about *A* which he has happened to observe, his facial expression, his movements, his words and actions, etc. The schema is not merely a mental portrait, for it includes all the trait-names which *B* may use in describing *A* to a third person. Moreover all of *B*'s own behavior with reference to *A* is influenced by it. The schema is, of course, biased. For *C*'s and *D*'s ideas of *A*'s traits are sure to differ from *B*'s ideas, and there is no means of deciding which are the most correct.

[2] D. S. Thomas, Some new techniques for studying social behavior, New York: Teach. Coll. Columbia, Bur. Publ., 1929. Pp. 203.

[3] Cf. G. W. Allport, Review of P. M. Symonds' Diagnosing personality and conduct, *J. Soc. Psychol.*, 1932, 3, p. 395.

A child has a different 'reputation' (May's useful term) [4] in the eyes of his teachers, his parents and his associates. Partly this bias is due to mere spatial and temporal limitations of acquaintanceship; *B* cannot possibly observe *A* throughout the whole of his existence. The schema is certain, also, to be over-simplified and stereotyped. The many-sidedness of *A*'s personality will be neglected, as Morton Prince [5] and June Downey [6] have pointed out; and hasty generalizations will be based on insufficient observation. Then there is the well-known phenomenon of halo, whereby a strong impression of one trait influences *B*'s judgments of all the other traits. A further feature of the schema is its consistency and integratedness. However correct or incorrect it may be, yet *B* will always interpret *A*'s words and actions so that they fit in with it. He believes that *A* behaves and will probably continue to behave in accordance with the traits that he attributes to him, and his own behavior toward *A* is likewise a consistent function of his schema.

The previous paragraph is well summed up by this quotation from Proust's *Swann's Way*.

> . . . , even in the most insignificant details of our daily life, none of us can be said to constitute a material whole, which is identical for everyone, and need only be turned up like a page in an account-book or the record of a will; our social personality is created by the thoughts of other people. Even the simple act which we describe as 'seeing some one we know' is, to some extent, an intellectual process. We pack the physical outline of the creature we see with all the ideas we have already formed about him, and in our minds those ideas certainly have the principal place . . . , so that each time we see the face or hear the voice it is our own ideas of him which we recognize and to which we listen.[7]

[4] M. A. May, and H. Hartshorne, Recent improvements in devices for rating character, *J. Soc. Psychol.*, 1930, 1, 66–77.

[5] M. Prince, Why we have traits—normal and abnormal: The theory of the integration of dispositions, *J. Abn. & Soc. Psychol.*, 1929, 23, 422–433.

[6] J. E., Downey, A suggestion for study of character-creation. *Proc. IX Intern. Cong. Psychol.*, Princeton, N. J.: Psychol. Rev. Co., 1930, pp. 149–150.

[7] Translated by C. K. Scott Moncrieff, p. 21 of the Modern Library edition, New York, 1928. It should be pointed out that 'intellectual' is used in this passage in the French rather than in the English sense; roughly speaking, it implies 'rationalization' (in the psychological sense) rather than 'rationality' (in the dictionary sense). Thus Bergson similarly contrasts the 'cinematographic abstractions of intellect' with the concrete truth that is perceived through 'instinct.'

Since a man's personality is different for every social group into which he enters, his traits, according to May,[8] should be regarded not so much as his own qualities as reaction patterns in the minds of his associates. This is the biosocial or *social stimulus value* theory of personality. May does not deny the existence of individual differences in reactivity; indeed a large part of his well-known work on character testing consists in the exploration of such reactivity, as distinct from mere social opinion. But he believes that personality cannot be completely described unless we take into account the individual as a stimulus to others. Certainly this view is supported by the evidence which we have just cited. Yet at the same time we must not ignore the naive, common-sense assumption that the traits are in some sense a property of the person. Hopeless confusion would ensue if we admitted that each observer's idea of a person was something entirely distinct. In spite of the diversity of our schemas, they usually overlap to a greater or lesser extent, and it is most important to find out why we agree at all in our judgments, or on what we agree. It may be true that traits are descriptive abstractions which express our generalized ideas of peoples' habitual behavior. But are they anything more than our personal generalizations; does the common element in a number of overlapping schemas correspond to something objective in the person himself, independent of his observers? To what extent is it possible to universalize and externalize the trait?

III. The Objectivity of Personal Characteristics at Different Levels

In setting out to answer these questions, we can safely state that people agree much more closely about some aspects of an individual than they do about other aspects, that some characteristics are more objective, some less so. Take height, for example. This is a name for a sense impression of a physical fact. Two or more judges may disagree in their estima-

[8] M. A. May, The adult in the community: Foundations of experimental psychology (ed. by C. Murchison), Worcester: Clark Univ. Press, 1929. Pp. 738–785.

M. A. May, The foundations of personality: Psychology at work (ed. by P. S. Achilles), New York: McGraw Hill, 1932. Pp. 81–101.

tions, but they will at once admit the truth if a yardstick is applied. Admittedly the readings of the yardstick in feet and inches are primarily in our own minds; they are introspective data. But so universal is our concurrence that, for most practical purposes, we can project these sense impressions onto the external world. Thus we are justified in concluding that height is a true property of the person himself; it is existential and biophysical, not biosocial.

An important distinction must be drawn between 'height' and 'tallness,' in spite of the fact that we sometimes use the terms interchangeably. Being tall is not merely a sense impression of length, because it is relative to other persons. Though based upon numerical height, it necessitates also a comparison in the mind of an external observer, and only acquires meaning when it is thus referred to some human standard. A 'short' European may be 'tall' among the pigmies of Central Africa. Tallness as such cannot be expressed in objective units, it is 'greater than' or 'less than' the average of the particular society in which the individual lives. And subjective factors sometimes distort its estimation, as when we think that a very 'dominating and overbearing' person seems 'taller than he really is.' Similarly old people often underestimate the tallness of young acquaintances, since they regard them still as children, not fully grown. Such disagreements, however, are mainly due to varying standards of comparison; it is seldom difficult to dissociate our judgments from our own minds, and to objectify them by translating them into terms of actual height.

Most physical capacities are like tallness. We say, for example, that *A* is a 'fast runner.' Now although his speed of running, namely x miles per hour, can be objectively measured in standard units, yet it has to be referred to some social norm, such as our own speed or the maximum human speed, before it acquires any meaning for us. The capacity is also complicated by intra-individual consistency, a factor which could be left out of account in discussing height. *A*'s speed will vary according to his degree of training, and according to the distance that he runs. Thus this apparently objec-

tive statement of a biophysical fact is largely tinged with subjective generalization and interpretation. *A*'s fast running cannot be regarded solely as his characteristic possession, in spite of its ultimate dependence upon his nerves and muscles.

Mental abilities introduce further problems. *A*'s intelligence is probably judged, in everyday life, by the standing he has obtained in academic endeavors, by the sort of books he reads, his quickness in following an argument, in understanding instructions, and so on. Long before statistical correlations told us so, we knew that individual consistency was high, but not perfect; that a person who has successfully completed a university education in classics, for example, will probably learn a modern language like French fairly easily, but will not necessarily be able to expound the theory of relativity. It would be rash, however, to state that mental abilities are really properties of the person, independent of the observer. Probably the better defined, more specific abilities can be successfully objectified, since they deal with types of behavior that are almost as unmistakable as running speed. A schoolmaster who dislikes a pupil may under-estimate his arithmetical ability; but by applying standard arithmetical sums to the whole class of pupils it would be easy to demonstrate to the master his error of judgment, for he would have to admit that arithmetical ability, to all intents and purposes, consists of success at such sums, relative to the norm for the population. 'Intelligence,' however, is much more vague and subjective, because different observers include under it such diverse modes of reactivity. Halo, stereotypy, or other forms of partiality can hardly be excluded. In fact we have to admit that a person's intelligence depends largely upon how his behavior impresses others. The only way to achieve universality is to agree that intelligence must be arbitrarily defined in terms of certain behavior such as that demanded by the Binet test (or its revisions). In actual practice this fairly objective criterion coincides rather closely with most observers' concepts of intelligence, though not so closely as the measures of a yardstick coincide with their concept of tallness.

IV. Can the Personality Trait be Objectified?

The preceding analysis should enable us to realize something of what we mean when we say that '*A* is tall,' '*A* is a fast runner,' '*A* is intelligent.' But the ground is still more uncertain when we ask what is meant by '*A* is irritable.' The following factors are all involved in the judgment of this trait-name, or of any other trait-name such as 'sociable, efficient, timid, gloomy,' etc.[9]

1. *Quality or type of behavior on the part of the subject, which we have learned to subsume under the trait-name.* The term behavior, as used here, includes not only directed actions and meaningful speech, but also expressive movements of the limbs, *Mitbewegungen*, facial expression, tone of voice, etc. It would be difficult to isolate any objectively definable reactions which could be taken as unmistakable criteria of irritability. Flushing, raising of the voice, incoördinated movements, clenching of the teeth, and so on, all occur in other emotional states besides that of anger. No robot or machine could record the fact that *A* is irritated in the same way that it could record *A*'s running speed. A qualitative judgment or interpretation of a complex pattern of words and reactions is essential.

2. *The duration and intensity or degree of overt reaction.* The following is a simple illustration. A waiter drops a plate while serving lunch to *A*, *B* and *C*. *C* laughs, *B* frowns and remarks to *C* that the waiter should be more careful, while *A* shouts abuse, drums on the table with a knife, and is only mollified when the proprietor of the restaurant comes to apologize. If we are watching at the next table, we are probably justified in judging that *A* is more irritable than *B*, *B* than *C*, because the more irritated a person becomes under a given stimulus, the more unmistakable is his overt behavior.

3. *The situation, or the quality and intensity of stimulation necessary to evoke this behavior.* We may all of us become irritated at some time in our lives, but not, perhaps, by so trivial an annoyance as that just described. In fact the nature of

[9] This list is based in part on a similar enumeration by L. G. Lowrey. The study of personality, *Amer. J. Psychiat.*, 1929, 8, 695–703.

the context or provoking stimulus is essential to a sure judgment of the trait. Here again objective description fails; it is 'the sort of things' which make *A* angry that counts. Although we commonly believe that a man's personality expresses itself in everything he does—even in his facial expression when he is at rest—yet it is far more clearly manifested when appropriate stimulation sets him in action. The 'real man,' it is said, is only discovered in times of stress. Continuing with the same illustration—*B* exhibited only mild signs of irritation at the incident; but if the waiter had dropped the plate and its contents on his head, and if his reaction had been the same as before, we should judge him to be a 'mild' rather than an 'irritable' person.

4. *The frequency and consistency with which this type of behavior is evoked in response to the same or similar situations.* Any single action is determined by so many conditions that all of us exhibit characteristics at different times which are, superficially, contradictory. *A*'s response to the waiter's clumsiness might be very different at the end of a satisfying meal from that at the beginning. And on a later, similar, occasion, when other persons happened to be present whom he wished to impress favorably for reasons having nothing to do with the meal, he might have suppressed all overt response. But exceptions do not necessarily lead us to doubt our judgment, especially when we think we know the motives behind them. No doubt, as the quotation from Proust showed, we tend unduly to ignore such inconsistencies as do not fit in with our stereotyped schemas. Only in pathological or near-pathological cases is the trait-behavior likely to be always 'true to type,' and in such cases, of course, the intensity of the behavior (*i.e.* factor No. 2) is usually exaggerated. The manic's excitability, Napoleon's dominating ambition, Tolstoy's humanitarianism color almost everything they do. In general, when we call someone by a trait-name, we mean that *more often than not* his reactions give us this consistent impression.

5. *The social norm. Would most people react to such a situation in such a way, with more or with less intensity?* Even

if we succeeded in translating the trait completely into terms of behavior, it would still not be an independent property of the individual, like his height. Rather it is a relative difference between him and the norm in the society of which the individual and his observers are members. As Kelley writes, "A certain Jew would have certain traits in Central Africa, others in the Ghetto in New York, and still others in Peking. This may seem confusing, since the person himself has not changed." [10] Similarly in some social groups *A*'s response, as described above, might be the normal thing, in which case other members of that group would not regard him as an 'irritable' person. Or a considerably higher intensity and frequency of the activity that is summarized under the trait-name (factors Nos. 2 and 4) would have to be exhibited in this social setting before the trait would be ascribed to him.

6. *The number of observers who agree that the behavior is more intense and frequent than the social norm.*

7. *The mental make-up and impartiality of these observers.*

These two factors can best be considered after we have attempted to summarize the above discussion in a phenomenal definition of a personality trait, as follows:

An individual is said to possess, or to be characterized by, a certain personality trait when he exhibits a generalized and consistent form, mode, or type of reactivity (behavior), and differs (deviates) sufficiently from other members of his social environment both in the frequency and intensity of this behavior, for his atypicality to be noticed by relatively normal and impartial observers, themselves members of this same environment.

This is to be taken only as a tentative, working definition.[11] It intentionally ignores the other side of our general problem, namely the psychological conditions which determine this 'consistent atypicality of reactivity.' All the factors outlined above are inter-dependently involved in the definition. Thus

[10] T. L. Kelley, Crossroads in the mind of man. Stanford, California: Stanford Univ. Press, 1928. P. 30.

[11] Professor G. W. Allport who, in spite of the divergencies from his viewpoint, has kindly read and criticized the manuscript of this paper, objects to the term 'atypicality,' on the ground that some people possess undistinguished personalities with no outstanding or extreme traits. But in such cases their mediocrity may be their main atypicality.

if the individual is transferred to a new social environment, while continuing to behave as before, then his atypicality may become more or less marked (cf. No. 5) and his traits, as observed by members of the new society, will certainly alter. This occurs, as we have already seen, even in respect of such definite characteristics as tallness, running speed, etc. We will therefore confine our attention to an individual in the social group in which he has lived most of his life.

Now the definition tries to express the notion that the trait is a relation between the individual and his observers. It is impossible to admit that the individual possesses an 'absolute' personality, entirely independent of more or less biased observers. His objective behavior, *i.e.* the data from which all his personality traits are judged, is distinctive and different from that of every other individual. His 'consistent atypicalities of reactivity,' however, are not his traits until they have been apprehended and interpreted by human mentalities. Thus the traits neither exist in the individual himself, nor merely in the minds of observers. The behavior is biophysical, the interpretations are biosocial, and both of these aspects, objective and subjective, are essential to the 'existence' of the trait. Nor can the trait ever be completely externalized or universalized because the schema or interpretation of every judge is partial and biased, and there can be no 'absolute' justification for accepting the schema of one judge rather than that of another. Each observer is himself a distinctive personality, and he necessarily selects only certain aspects of the total reactivity in developing his schema, hence he can never be, in the words of our definition, entirely 'normal and impartial.' We can, however, allow that the more marked and consistent an individual's atypicality, the more likely are the various schemas of him to be based on reliable observations of objective behavior, and the more universal will be the characterizations. Thus the personality traits of the manic patient are fairly unmistakable. He behaves in such a way that almost all judges will agree in their opinions, and we are justified in accepting such majority opinion as something truly characteristic of the patient.

It is clear, then, that the ordinary traits of normal people will never be amenable to purely objective scientific study. Nevertheless this does not imply that all attempts to investigate personality will be fruitless; it indicates that the most successful approach should make use both of objective and of interpretative methods, along the following lines. A comprehensive account of the whole of the reactivity of the individual should be collected, including the facts of his life-history together with the opinions of many different observers, all of whom will already have interpreted in their own way such portions of his reactivity as were available to them. Drawing on all these data, a fairly adequate interpretation of his personality should be possible. To take a concrete example: Modern historians who have no particular axe to grind are probably better capable of assessing Napoleon's traits than were any of his contemporaries. In spite of their closer contacts with him, the latter could not know so many relevant facts as we know, and they were probably less detached.[12]

V. The Ambiguity of Trait-names

Traits are names for very general classes of behavior; as soon as we start to define them we find that their content is extremely ambiguous, and that they cannot be precisely distinguished from one another. A number of additional characteristics of the trait follow from this ambiguity, all of which confirm us in our conclusion that it cannot be accepted as a biophysical entity.

Each individual's reactivity is an organized, adaptive whole, hence it cannot readily be split up into discrete types which might be labelled in isolation from one another. The following is a rough analogy. Suppose that we are shown a collection of all the flowers in the world, and are told to try to name the color of each one. We possess only, say, twenty different names, so that we soon find ourselves forced to call by the same name a great number of flowers of distinct shades. Other observers probably classify them rather differently from

[12] *Cf.* P. E. Vernon, Human temperament, *Eug. Rev.*, 1932, 23, 325-331. This article embodies a more succinct account of the thesis which is developed in the present paper and which, it is hoped, will be followed up in later publications.

ourselves. Certainly we cannot assign them to separate homogeneous groups of red flowers, blue flowers, yellow flowers, etc., since there are just as many intermediate colors, purples, oranges, and even hundreds of specimens intermediate between purple and blue, orange and yellow, and so on. In much the same way the reactivity of every individual is of a different qualitative shade from that of every other, and our names are too static and cinematographic to cover all this diversity. True, we possess more trait-names than color-names, but they are more equivocal, and they overlap in meaning to a greater extent even than red, scarlet, crimson, etc. It is impossible to decide on dividing lines between, say, 'tolerant, lenient, liberal, benevolent, indulgent, kind-hearted, charitable, philanthropic, and munificent' behavior. Munificence, according to our present language conventions, may describe a different sort of behavior from tolerance, but no two adjacent epithets in the series are really distinct, either in the minds of observers, or in the reactivity of the observed. Nor are there any available criteria for determining which are the more fundamental and distinct. For example, 'introversion' might be resolved into 'reclusiveness' and 'day-dreaming'; equally well 'reclusiveness' might be resolved into 'unsociality' and 'narcissism,' and so on.

Certain aspects of a person such as height, weight, sex, sensory acuities, physical abilities, etc., do correspond to absolutely distinct types of experience. And in general, mental abilities, interests and attitudes are fairly well-defined, because they are crystallized around certain objective spheres of activity. Mathematical aptitude, interest in music, pacifism and the like are far more discrete than our interpretative trait-names, since they refer to objects, institutions, cultural entities, etc., and therefore mean much the same to all of us. But traits often mean different things to different people. Hence the same item of behavior may often be classified under varying trait-names. The following illustration is typical. "A nursery school director rated a child high in 'reverence' because she found him gazing fixedly for a long time at a flower. Another person might have called this

behavior introversion, another persistence, or good attention-span, or dullness of interest, or curiosity, or love of beauty." [13] Again, although both the aptitude, the interest and the trait are deduced by generalizations from observations of a large number of items of behavior, yet the items which go to make up aptitude for mathematics or interest in music possess many objective elements in common; whilst the connection between the items of behavior which we summarize under a trait-name is largely subjective and interpretative.

In conclusion, it should be pointed out that the present paper does not attempt to deal with the biological or psychological determinants of the individual differences in reactivity which are interpreted as personality traits. Most previous writers have been concerned almost exclusively with these biophysical aspects, to the neglect of the biosocial aspects. One or two suggestions for a future theory which will cover both sides of the topic may be deduced from the above discussion.

(*a*) Since the personality trait is essentially dependent upon the interpretation of human reactivity by external observers, and since it varies in different social settings, the postulation of a biophysical drive or system of energy (like an instinct) in the organism, to account for each trait, would seem to be unjustifiable.

(*b*) Since the connection between the items of behavior which go to make up a trait depends on the subjective meaning of these items, it is impossible to achieve a satisfactory definition of a trait in terms of 'habits' which contain 'common identical elements.' (This is the stock alternative theory of those who reject the dynamic general trait theory.)

(*c*) Since traits are names for very general classes of behavior, they cannot be expected to fit at all adequately the extreme complexities of an individual's reactivity. Hence the apparent inconsistencies which the mental tester discovers in personality are due to the artificiality of these names, rather than to any fundamental lack of integration in the individual.

[13] J. N. Washburne, Definition in character measurement, *J. Soc. Psychol.*, 1931, 2, 114–119.

VI. Summary

1. Personality traits, in spite of their subjectivity, cannot be eliminated from the concrete study of the personalities of human individuals. A rigidly objective approach to this study, which rejects all interpretation, is barren.

2. An observer's notions of, and reactions to, the traits of an individual may be termed his *schema* of that individual's personality. The schema is necessarily partial, both because the observer's knowledge is limited, and because of halo effects, etc.; it is dependent, also, upon the personality of the observer himself. But however incorrect it may be, the observer interprets all the individual's words and actions to fit in with his schema.

3. Since different observers' schemas of the same individual always differ, May's *social stimulus value* theory considers that the individual's traits are not existential (*i.e.* his own properties), but biosocial (*i.e.* impressions in the minds of others).

4. Some of the individual's characteristics, height for example, are clearly more objective and biophysical than others. Motor abilities and mental aptitudes are objective in so far as they can be translated into well-defined behavior. But they cannot, like height, be expressed in absolute scientific units; they are relative differences from a social norm. Moreover they are inexact generalizations, since they do not allow for the individual's differential abilities both at different times, and in different specific contexts.

5. In the determination of personality traits these difficulties are augmented. The behavior which is to be included under the trait-name is more complex and ill-defined. Both the duration, intensity, frequency and consistency of such behavior have to be taken into account, and the qualitative nature of the situation or stimulus which evokes the behavior.

6. A personality trait is never wholly objective since it must be considered as a relative difference or atypicality between the person and the social group of which he is a member. And the same objective behavior may be interpreted differently by different observers. Nor can the behavior it-

self be identified with the traits until it has been so interpreted. The trait is both biosocial and biophysical, a relation between the person and his observers.

7. The ambiguity in the behavioral content of a trait, as contrasted with aptitudes, interests, etc., renders impossible the isolation and demarcation of any absolute criteria for its existence. A person's total integrated reactivity cannot be split up into discrete and easily recognizable or measurable types.

8. Traits must be used as interpretative names for certain general classes of reactivity, or aspects of our schemas. They overlap with one another both in the behavior that they cover and in our language conventions. Apparent inconsistencies in personality are due to the over-simplifications introduced by applying these stereotyped trait-names.

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THE CONUNDRUM OF THE CONDITIONED RESPONSE

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A survey of the literature on the conditioned response doctrine with especial reference to the temporal relationship between the presentation of the conditioned stimulus and the unconditioned stimulus makes it at once apparent that there are many questions of experimental import that remain unanswered and unchallenged in the literature on the subject. In an investigation involving the conditioned response there are three possible time relations between the presentation of the conditioned and unconditioned stimuli. They may be presented in exact coincidence and the resulting phenomenon is known as 'simultaneous conditioning'; the conditioned or inadequate stimulus may precede the unconditioned stimulus (variations of this may result in the 'trace' or 'delayed' reflex) and the resulting conditioning is termed 'forward conditioning'; and the conditioned stimulus may be presented after the unconditioned stimulus and this results in 'backward conditioning.' In any problem of conditioning the investigator immediately faces at least three distinct problems: What is the most efficient temporal relation between the conditioned and unconditioned stimuli? Are 'forward,' 'simultaneous,' and 'backward' conditioning equally efficient? And are any of the three incapable of producing conditioning? The data on this problem are inadequate and at variance, as a perusal of the different reports, based upon experimental evidence, readily shows. This paper will attempt to summarize and re-envisage the more important investigations of this particular problem and advance a few of the possible explanations as to why this confusion exists in the present status of the research.

The law of contiguity, advanced by many of the members

of the British Association School, is regarded as the forerunner of the modern work on the conditioned reflex. 'Simultaneous' and 'successive' association lent itself to many investigations chiefly concerned with memory material and learning. The results of these studies may be summarized by the work of Ebbinghaus, Müller, Pilzecker, Wohlgemeuth, and others, in which evidence is presented to show that association may function in the backward direction, but its strength is never equal to that of the forward tendency.

The initiation of the laboratory technique on the conditioned salivary response in the dog by Pavlov (7) and his students in the early part of the twentieth century resulted in the positive statement "It is necessary that the action of this phenomenon on the dog should occur simultaneously with the unconditioned reflex" (p. 101). This occurred in a publication in 1908 and is to be found again as late as 1928 when the following statement was made before the Royal Society in the Croonian lecture, "A full and stable conditioned reflex develops when the stimulus which is to become conditioned slightly precedes that activity (unconditioned reflex) with which it is to be linked, . . . but if the stimulus is introduced *after* the beginning of the activity, then, although, as our present experiments seem to show, a conditioned reflex may also develop, it is insignificant and evanescent" (p. 381).

A summary of the experiments upon the conditioned response soon convinces one that there are enormous differences in the speed of formation and the quantity of the response. Again, Pavlov states "There are numerous details concerning the speed of formation of the conditioned reflex. . . . As the conditioned reflexes are in the highest part of the central nervous system, in which there is a constant collision of innumerable influences from the external world, it is comprehensible that among the different conditioned reflexes there is an incessant struggle, a choice among them at any given moment. Consequently there are constantly arising cases of inhibition among these reflexes" (p. 125).

Yarbrough (13) studied the withdrawal movements of rats to an electric shock and reported that simultaneous pres-

entation of the stimuli was the most effective in bringing about conditioning. If the conditioned stimulus followed one second after the shock, practically no conditioning was secured. If it occurred immediately after (no time noted) it was found to be more efficient than if the conditioned stimulus preceded the shock by an interval of two seconds. "Association in the backward direction is very little, if any, more difficult than in the forward direction when the continuous mode of presentation is used. . . . If in succession an interval of one second is used, reverse association is perhaps impossible."

Carr (1) attacked the problem of successive association by requiring rats to run alternate paths in a maze in which the time between the runs was varied by the time allowed for eating. The intervals used in Experiment 1 were 15, 25 and 35 seconds. No differences in the curves were found as to the speed of the formation of the association. In Experiment 2 intervals of 5, 10 and 15 seconds were used, in which the 10-second interval was found to be possibly more efficient. The number of trials per day seemed to present the best evidence of the difference between the groups rather than the length of the interval. "Our results do not permit of any statement that the rate of learning is a function of the intervals employed. . . . One is not justified, however, in extending this conclusion to other intervals." Carr admits that "the method of the experiment is not particularly well adapted to the solution of the problem."

Carr and Freeman (2) investigated the problem of the time relationship relative to rats running a maze. Three intervals were used, a buzzer was sounded one second before the rat reached the end of a blind alley, simultaneously with the reaching of the end of the blind alley, and one second after reaching the end of the blind alley. They presented evidence to show that the forward conditioning in which the one-second interval was used was most favorable for conditioning. Practically no conditioning occurred in backward direction and the statement follows that it is "perhaps impossible to obtain backward conditioning."

Cason (3, 4) in an extended series of investigations on the conditioned eyelid reflex and the conditioned pupillary reflex concludes that there is no decisive evidence for forward conditioning in the pupillary reflex and positive evidence against backward conditioning. In the eyelid reflex he states that there is some evidence against backward conditioning although it is very inconclusive. In both experiments the simultaneous presentation of the stimuli was found to be most efficient.

In an investigation of the patellar reflex, Schlosberg (8) found the greatest efficiency in the forward direction. The intervals between .20 and .44 seconds brought about maximum efficiency in setting up a conditioned reflex. If the interval was decreased below .11 second, it was found to be more difficult to produce a conditioned response.

In an investigation of the lid reflex, Switzer (9) found backward conditioning possible in 18 out of 20 subjects. Backward conditioning was most readily established when the conditioned stimulus followed the unconditioned stimulus at intervals of one-half to one second, "but it also occurred in some subjects when the interval was extended to two seconds." In a supplementary study of the patellar reflex, backward conditioning was secured in two out of five subjects while backward conditioning was not established in tests made on five subjects in an investigation of the galvanic skin reflex.

More recently Wolfle (11, 12) has investigated the conditioning of the finger withdrawal to an electric shock. The temporal relation of the conditioned stimulus and the unconditioned stimulus ranged from .50 seconds to 1.50 seconds by intervals of one-quarter seconds. The .50 second interval in the forward direction was found to be the most effective in producing the conditioned withdrawal movement. Conditioning in the backward direction was obtained but it was of very low efficiency. Sufficient intensity was used to cause a withdrawal of the finger and was gradually increased as the practice period became longer. The intensity of the induction shock was changed during the course of the experiment if the subject did not withdraw the finger when shocked or if the

finger was placed on the electrode while the switch was still on. Many of the subjects were never conditioned, bearing out the general statement that conditioned reflexes are unstable and difficult to obtain in man.

The work by other investigators in this field only adds to the confusion of the problem. The greatest amount of congruence seems to be in reference to the inadequacy of backward conditioning; even though some report it possible, it is unstable and evanescent.

The following chart illustrates the findings of some of the research workers who have either directly or indirectly referred to this specific problem.

The laws of the formation of the conditioned response as stated in textbooks and the general conclusions usually cited under this concept leave one with the impression that certain fundamental findings have proved in all instances to hold for conditioning as certain representative authors such as Hunter (**6**), Watson (**10**), and Dashiell (**5**) seem to indicate. Hunter (p. 622) states that "If, for example, the presence of food in the mouth causes saliva to flow and if it is desired to produce that result with an auditory stimulus, the latter must either be presented simultaneously with the food or slightly before the food. Presentation subsequent to the food will not result in the conditioned response." Evidently many factors are at work which the various investigators have failed to take into account or there is a gross violation of some of the fundamental principles of scientific research. Statements to the effect that one method is superior to the other and that one interval is more efficient than another are without any foundation except under the specific experimental conditions under which the individual investigator worked.

The experimental procedures and the control of the various constants are widely divergent in their methods of approach to this problem. In the first place, the conclusions as to the superiority of one temporal relation over another has been based upon the experiments performed on a variety of organisms. Different receptor mechanisms, widely different neural organizations and different effectors play their part in com-

THE TEMPORAL RELATIONSHIPS IN THE CONDITIONED RESPONSE

Author	Effector	Forward Conditioned stimulus precedes unconditioned	Simultaneous	Backward Unconditioned stimulus precedes conditioned
Pavlov	Salivary gland (dog)	Possible	Most efficient	Not possible in earlier work. 1928—Possible but insignificant and evanescent
Krestovnikoff	Tactual (dog)	Possible	Efficient	Not successful in 1,000 trials
Cason	Eye pupil (human)	No conclusive evidence	Most efficient	Positive evidence against
Cason	Eyelid (human)		Most efficient	Some evidence against
Dashiell	Effectors in general	Efficient	Efficient	Not possible
Watson	Effectors in general	Efficient	More efficient	Not possible
Yarbrough	Rats (learning)	Possible	Most effective	Practically none with interval of 1 second after shock
Switzer	Eyelid (human)	Possible		Possible in 18 out of 20 subjects. ½ to 2 seconds
Switzer	Knee jerk			Secured in 2 out of 5 subjects
Switzer	Galvanic skin reflex			Not established in tests on 5 subjects
Wolfle	Finger withdrawal	Most efficient ½ second interval	Possible	Possible, but unstable
Carr & Freeman	Rats (withdrawal)	Most efficient	Possible	Slight evidence. Perhaps impossible
Schlosberg	Knee jerk	.20 to .44 seconds most efficient	Possible	

plicating the problem. This means that a variety of reflexes have been studied under many different conditions and the fact that conditioned responses have been established on conditioned responses, *e.g.* salivary reflex, implies that isolation of

variables has not been taken into account. Unanimity of conclusions must take these factors into account.

That the intensity of the various stimuli used determines to a large extent the time required for establishing a conditioned reflex was recognized very early by the Pavlovian school. With the conditioning of the temperature reflexes it was found that the conditioned reflex was established at 0° C. or 1° C. in 20 or 30 trials, whereas when a temperature of 4° or 5° C. was used, no effect was produced in 100 trials. Again, 45° C. gave no action after 100 applications but a temperature of 50° C. resulted in conditioning in 20 or 30 trials.

Likewise Pavlov (7) noted the differences in sound intensities in the establishment of the conditioned reflex. "We must state with regard to acoustic stimuli that very loud sounds such as the violent ringing of the bell, did not, in comparison with weaker stimuli, quickly establish a conditioned reflex." Not only does the intensity influence the establishment of the conditioned reflex with reference to acoustic stimuli, but the pitch and quality of the tone were also found to influence the rate of conditioning.

The different experimental procedures have not taken into account the influence of the differences in intensities in both the conditioned and the unconditioned stimuli and their influence upon the rate of conditioning and upon the optimum temporal interval between the two stimuli. Neither has the problem been investigated where one stimulus is very intense and the other weak or both intense or both weak. The difficulties of this problem are almost insurmountable as there is no adequate criterion for equating a tone of a certain intensity value with an induction shock of a certain intensity value providing these two stimuli are used in the process of setting up a conditioned response. The intensities of the various stimuli must be objectively equal from one experiment to another before the superiority of one temporal relation can be established. It is probably true that efficiency varies with the intensities used, but it is our contention that in order to draw *general conclusions* on the conditioned response technique

a more efficient control over intensity must be maintained. That fatigue influences the results obtained in regard to this question is evident especially as two methods of presentation of the two stimuli are employed, the successive and continuous. The various investigators have not mentioned this fact in an interpretation of the optimum temporal interval necessary to produce a conditioned reflex. This problem is closely related to the refractory phase which is discussed below.

The ages of the subjects have not been taken into account in an interpretation of the results. Especially since infra-human and human subjects have been used in a determination of the temporal interval best suited for setting up conditioning, there is no standard of reference as yet which throws any light on this problem. Individual differences in the different species and among the same species, and between the sexes also enter to complicate the problem, but no one has presented experimental evidence to show that these conditions were taken into consideration from one experiment to another.

The physiological work on the refractory phase and its influence upon the excitability of nerve and muscle tissues presents excellent evidence of the individual differences of protoplasmic irritability, especially in an interpretation of the conditioned reflex doctrine. That this is a phenomenon present in all tissues has been amply demonstrated by many investigators. In the reflexes the refractory phase varies with the complexity of the neuromuscular mechanism involved, thus behaviors involving the cerebral cortex would present the most variability and greatest extent in refractory phase. The lid-reflex has a refractory phase of three seconds or more, the knee jerk a refractory phase of one-half second and nerve fibre possesses the least refractory phase; they are of the order of a few thousands of a second. In finger movements the refractory phase as measured by the maximum speed of oscillatory movement is of the order of one-eighth of a second. That the refractory phase is without a doubt one of the fundamental elements determining the optimum temporal interval of the conditioned and unconditioned reflexes, especially in trace and delayed reflexes, must not be denied.

In fact, the most probable explanation seems to be within this realm. The interplay and interdependence of the refractory phases of tissues possessing different time constants is important in the investigation of any conditioned reflex research. It is an important item in the determination of the relative superiority or even the possibility of forward, simultaneous, or backward conditioning. It may be that one time relation is possible in one combination of tissues and not possible in another set, especially since many of the experimental conditions are unknown or permitted to vary at will.

All of these factors show the extreme delicacy and manifold considerations necessary in a study of a problem of this type. The necessary delicate technique, the prevention of diffusion of stimuli in addition to a knowledge of the factors which heretofore have been allowed to vary at random means that at the present time to state broad generalizations in regard to the most efficient, the possibility or impossibility of certain time relations in the establishment of a conditioned reflex, is unwarranted and unscientific.

We are not prepared to maintain that objective equality of the various factors from one experiment to another will answer the above objections. The one factor alone of the relation of the intensity of the stimulus to the magnitude of the response is itself a highly complicated problem involving many factors at which we can only guess. But consistency and uniformity of conditions must be strived for, if we are to make any progress in our generalizations which are based on the research technique.

It is safe to say that at the present time it is impossible to draw any definite conclusion in regard to the most advantageous temporal interval in the establishment of the conditioned reflex.

Wolfle (13) suggests that "the experiment should be repeated with other intensities, with different conditioned stimuli, and with other unconditioned reflexes," and says that her findings in the withdrawal response of the finger to an electric shock are at variance with other investigators. "The zero interval, or simultaneous relationship, also resulted in

very little conditioning (10 per cent). *This is contrary to the statement in most textbooks, that the simultaneous application of both stimuli is the best possible arrangement of them*" [1] which is a rather premature generalization to make on an experimental problem which has at least the following variables: variety of organisms, variety of reflexes, conditioning on conditioned stimuli, intensities of the various components unknown, as likewise the quality, duration, and variability of these during the experimental procedure, fatigue, age, individual differences, refractory phases of various neuromuscular mechanisms.

Further experimentation upon this important problem must strive for the control and measurement of the above factors if we are to have a satisfactory answer as to the possibility of securing conditioning in any of the three possible time relationships. We must also take these factors into account when we present evidence for the *optimal* temporal relation of the two stimuli.

Summary

The literature concerning the experimental work on the establishment of the conditioned reflex shows a wide variety of results with especial reference to the temporal relationship of the conditioned stimuli and the unconditioned stimuli. Various investigators have presented evidence concerning the optimal temporal relation of the two stimuli, as well as the possibility or impossibility of conditioning in any of the three possible time relations, and all have been at variance. This paper attempts to show that there will be no unanimity of results until a number of experimental procedures have been equated. No adequate generalized statement concerning the temporal relationship in the establishment of the conditioned reflex can be made at the present time in view of the experiments carried on thus far.

[1] Italics mine.

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